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TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

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TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

VOL. X.

KING'S COLLEGE HOSPITAL

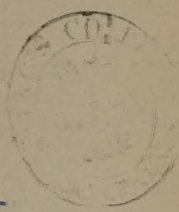
MEDICAL SCHOOL.

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[The figures prefixed denote the date of election. The figures appended to Names denote the number of Communications. Original Fellows are marked †].

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- 1890 STEWART, JAMES S., L.R.C.P., L.R.C.S., House Physician Richmond, Whitworth, and Hardwicke Hospitals.
- 1884 STRAHAN, MICHAEL, L.R.C.S., Medical Officer, No. 2 North City Dispensary District, 38 Rutland-square, Dublin.
- 1887 THOMPSON, W. H., M.D., F.R.C.S., Eng., Physiological Laboratory, University College, London.
- 1891 THOMPSON, WILLIAM JOHN, L.R.C.P., L.R.C.S., 15 Harrington street.
- 1890 TRUMAN, CLAUDE A. P., L.R.C.P. Ed., Charnwood, Kendrick-road, Reading, Berks.
- 1889 TWEEDY, ERNEST HASTINGS, L.R.C.P., L.R.C.S., House Surgeon, Steevens' Hospital, Dublin.
- † WHITE, WM. DUDLEY, L.R.C.S., Medical Officer, No. 3 Dispensary District, North Dublin Union, 51 Rutland-square, Dublin.
- 1891 WOODS, ROBERT H., M.B., B.Ch., Throat Surgeon, Richmond Hospital, Upper Merrion-street, Dublin. [2]
- 1887 WYNNE, GEO. NESBITT, M.D., M.Ch., 77 Aungier-street, Dublin.

STUDENT ASSOCIATES.

- BOURKE, JOHN J., Medical School, Cecilia-street, Dublin.
- CUMSTON, C. G., Geneva.
- FERGUSON, J. H., 4 Usher's Island.
- M'CUTCHEON, M. A., 16 M'Mahon-street, Dublin.
- M'GEE, T. H., Medical School, Cecilia-street, Dublin.
- O'DONNELL, BRYAN L., 14 Adelaide road.
- TIGHE, J. M., Dublin.

RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and eight Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.^a

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

- 8A. That all Rules referring to the admission of Fellows, Members, and Student Associates shall be interpreted as referring to Ladies as well as Gentlemen.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

^a Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Medical Officers of the Army and Navy, and Registered Medical Practitioners not residing within 15 miles of Dublin, are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion — five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, and the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section ; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888.)

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy ; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy ; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the other Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years. (1888.)

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the “Transactions” is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the

General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology. (1888.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year.

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for office, provided that the Fellow so nominated shall have consented to act. (1891.)

36. That all elections shall be by ballot.

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the Meeting. (1888.)

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members at least three weeks previously. (1891.)

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

58 (a). A paper by any other than a Fellow or Member of the Academy shall not be read before the Academy unless the author of such a communication shall have obtained permission to do so from the Council of the Section before which the communication is proposed to be read. (1892.)

Ordinary Meetings.—Order of Business.

59. (1.) Chair to be taken at 8 30 p.m.
- (2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.
- (3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.
- (4.) There shall be no Exhibition of Specimens by card in the Obstetrical or Pathological Sections.
- (5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynaecology.
- (6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President shall invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.
- (7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.
- (8.) Chairman to call upon the author of the first paper on the list to read his paper.
- (9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.
- (10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.
- (11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

60. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.
- (2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.
- (3.) Specimens must be in the room at 7 45 on the night of Exhibition.
- (4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

- (5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.
- (6.) The Exhibitor should be present, and he shall furnish further details if asked for.
- (7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

61. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889).

By-laws concerning "Transactions."

62. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

63. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications, subsequent to the reading of the same before the Academy, in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read. (1891.)

64. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

65. The "Transactions" may be purchased by Members at cost price.

66. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

67. Contributors of papers are requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof. (1888.)

68. That on the evening of the day of meeting of the Sectional Council, when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

- (1.) That he is expected to be ready or else take his place at the bottom of the list.
- (2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

69. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

70. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

71. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

72. New Laws, or alterations in existing LAWS, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

R E P O R T .

THE General Council begs to report that the number of Fellows for the Session 1890-91 was 243; of Members, 27; of Students, 12. The Fellows have increased by 6, the Members decreased by 2, and the Students decreased by 9.

At the beginning of the Session, the General Council received resolutions from the Councils of the Medical, Obstetrical, and Pathological Sections on the subject of reporting the Discussions, and keeping a permanent record of them. There was a difference of opinion as to the changes which should be made, and the Council referred the whole subject to a Sub-Committee, consisting of Sir W. STOKES, Dr. J. W. MOORE, Dr. F. W. KIDD, and Mr. THOMSON.

This Committee recommended—1st. That the Discussions be printed each year, at the close of each paper, at the end of the Volume, or in a separate small volume. 2nd. That there be no monthly issue, at least for this Session, seeing that the proceedings are reported in all the Medical Journals. 3rd. That reporting by professional reporters be discontinued for this Session, and that slips be provided on which speakers at the meetings shall be requested to write the substance of their remarks before leaving the room. That these slips shall be at once handed to the Secretary of the Section, and shall, with the Abstract of the Paper read, form the report of the proceedings.

The Council, having regard to the discussion at the General Meeting as to reporting, resolved to adopt the recommendation of the Committee on that subject, and they further agreed to print the Discussions in the "Transactions" at the end of each paper.

Professional reporting has been dispensed with since February, 1890,

The Council has thus endeavoured to carry out the wishes of the Fellows as expressed by some of the Sectional Councils and at the General Meeting, and the forthcoming Volume will contain the Discussions.

The extra expenditure this year includes £50 for microscopes, and £26 5s. to Professor PURSER for his lecture to the Academy, which will also appear in the "Transactions." The publication of the last Volume was also dearer by £27 than the preceding one. The Council have not, therefore, been able to add to the Investment Fund this year, but they believe that they will be in a position to do so during the present Session.

It was reported to the Council that a paper had been published in an American periodical before it was read at the Obstetrical Section, and the Secretary communicated with the author. It was explained that the premature publication arose through an unexpected delay in the reading of the communication.

In order to prevent a recurrence of such an irregularity, the Council recommends that Rule 63—"All communications accepted by the Academy become the property of the Academy, but authors may also print the communications in any publication in addition to the 'Transactions,' &c."—shall be amended by inserting the words, "subsequent to the reading of the same before the Academy," after the words "their communications."

The Council desires to record with deep regret the deaths of Dr. ANTHONY H. CORLEY and Dr. LENNOX BIGGER, original Fellows of the Academy. Dr. CORLEY contributed much to the "Transactions," and always took an important share in the meetings of the Academy.

SAMUEL GORDON, *President.*

WILLIAM THOMSON, *General Secretary and Treasurer.*

General Treasurer's Report for the Session 1890-91.

RECEIPTS.

	£	s.	d.
To Balance in Bank, Oct. 14th, 1890	£5	7	1
" In Treasurer's hands	-	0	18
		<u>0</u>	
" Subscriptions	-	-	6
" Dividends on Consols (£491 16s. 9d.)	-	495	18
" Fannin & Co., refund on Illustrations	-	15	19
			9
			1
			5

Total - - - - £520 0 3

We have examined the Treasurer's Statement of the Accounts of the Royal Academy of Medicine for the Session 1890-91, and we find it correct.

October 13th, 1891.

EXPENDITURE.

	£	s.	d.
General Secretary	-	-	105 0 0
Leitz—Microscopes	-	-	50 0 0
Carriage do.	-	-	0 15 10
Professor Purser—Lecture	-	-	26 5 0
Delft	-	-	0 19 4
Reporters	-	-	14 14 0
John Falconer, Printing Slips, &c., Vol. VIII.	-	-	25 0 0
Fannin & Co., Printing Vol. VIII. and Postage	-	-	175 0 0
Attendants	-	-	18 10 0
Refreshments	-	-	21 11 6
Stationery	-	-	4 14 6
Illustrations	-	-	4 19 11
Printing Circulars and Postage	-	-	60 13 6
Cheque Book	-	-	0 4 2
Balance in Bank	-	-	11 12 6

Total - - - - £520 0 3

{ G. P. L. NUGENT.
A. W. W. BAKER.
WM. THOMSON, General Secretary and Treasurer.

Volume IX. of the "Transactions" has been forwarded to the following:—

Lancet	-	-	-	-	London.
British Medical Journal	-	-	-	-	Do.
Medical Press	-	-	-	-	Dublin.
Hospital Gazette	-	-	-	-	London.
Dublin Medical Journal	-	-	-	-	Dublin.
London Medical Recorder	-	-	-	-	London.
Edinburgh Medical Journal	-	-	-	-	Edinburgh.
Glasgow Medical Journal	-	-	-	-	Glasgow.
Liverpool Medical Journal	-	-	-	-	Liverpool.
Bristol Medical Journal	-	-	-	-	Bristol.
Asclepiad	-	-	-	-	London.
International Medical Journal	-	-	-	-	Do.
Annals of Surgery	-	-	-	-	Do.
Provincial Medical Journal	-	-	-	-	Leicester.
Birmingham Medical Review	-	-	-	-	Birmingham.
Sanitary Record	-	-	-	-	London.
Practitioner	-	-	-	-	Do.
College of Surgeons	-	-	-	-	Dublin.
Do.	-	-	-	-	London.
Do.	-	-	-	-	Glasgow.
Trinity College	-	-	-	-	Dublin.
Royal University	-	-	-	-	Do.
Queen's College	-	-	-	-	Belfast.
Do.	-	-	-	-	Galway.
Do.	-	-	-	-	Cork.
Royal Dublin Society	-	-	-	-	Dublin.
College of Physicians	-	-	-	-	Do.
Do.	-	-	-	-	Edinburgh.
Do.	-	-	-	-	London.
Faculty of Physicians and Surgeons	-	-	-	-	Glasgow.
Victoria University	-	-	-	-	Manchester.
University College	-	-	-	-	London.
Harveian Society	-	-	-	-	Do.
Pathological Society	-	-	-	-	Do.
Clinical Society	-	-	-	-	Do.
Medical and Chirurgical Society	-	-	-	-	Do.
Apothecaries' Hall	-	-	-	-	Do.
King's College	-	-	-	-	Do.
University	-	-	-	-	London.
Do.	-	-	-	-	Glasgow.
Do.	-	-	-	-	Edinburgh.
Do.	-	-	-	-	Durham.
Do.	-	-	-	-	St. Andrews.
Do.	-	-	-	-	Aberdeen.

University College	-	-	-	-	Dundee.
Do.	-	-	-	-	Melbourne.
Do.	-	-	-	-	Sydney.
Do.	-	-	-	-	Adelaide.
Do.	-	-	-	-	Calcutta.
Do.	-	-	-	-	Bombay.
Do.	-	-	-	-	St. Petersburg.
Do.	-	-	-	-	Paris.
Do.	-	-	-	-	Vienna.
Do.	-	-	-	-	Berlin.
Do.	-	-	-	-	Brussels.
Do.	-	-	-	-	Bologna.
Do.	-	-	-	-	Madrid.
Do.	-	-	-	-	Amsterdam.
Do.	-	-	-	-	Christiania.
Do.	-	-	-	-	Stockholm.
Do.	-	-	-	-	Toronto.
Do.	-	-	-	-	Quebec.
Do.	-	-	-	-	New York.
Do.	-	-	-	-	Philadelphia.
Do.	-	-	-	-	New Zealand.
Do. Library	-	-	-	-	Tokio, Japan.
Medical Institute	-	-	-	-	Birmingham.
Dr. Ashby	-	-	-	-	Do.
Director-General Billings	-	-	-	-	Washington, U.S.
Journal de Médecine et de Chirurgie (M. Lucas Championnière)	-	-	-	-	Paris.
Archives de Chirurgie	-	-	-	-	108 Boulevard St. Germain, Paris.
Archiv für Klinische Chirurgie	-	-	-	-	Berlin.
Centralblatt für die medicinischen Wissens- chaften	-	-	-	-	Do.
Zeitschrift für Chirurgie	-	-	-	-	Do.
The Australian Medical Gazette	-	-	-	-	Sydney, N.S.W.
Spitalul	-	-	-	-	Bucharest.
New York Medical Journal	-	-	-	-	New York.
Journal, American Medical Association	-	-	-	-	Chicago.
Bulletin de l'Académie Royale de Médecine de Belgique	-	-	-	-	Brussels.
Medical Journal	-	-	-	-	Brooklyn.
Naturforschende Gesellschaft, Dr. Rudolf Martin, Seefeldstrasse 119, Zurich.					
Academy of Medicine, 17 West 43d Street, New York.					
Library, College of Surgeons, Edinburgh.					
“ British Medical Journal, 429 Strand, London.					
“ Medico Chirurgical Society, Bristol.					

TRANSACTIONS
OF THE
ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF MEDICINE.

RECENT ADVANCES IN THE ÆTIOLOGY OF
DISEASES OF THE SKIN, AND THEIR BEARING
UPON TREATMENT.

BY WALTER G. SMITH, M.D.;

Physician to Sir Patrick Dun's Hospital;
King's Professor of Materia Medica, School of Physic, Trin. Coll. Dubl.

[Read in the Section of Medicine, November 20, 1891.]

IN the present paper I propose to collect and summarise the results of some recent investigations. I shall endeavour to show the light which these doctrines and inquiries have thrown, and are calculated still more in the future to shed, upon the group of diseases of the skin.

No attempt at a complete survey will be made, nor would, indeed, be possible in the time; and a restricted field—that of cutaneous diseases—is purposely chosen, for the sake of illustration, although it will be obvious that the facts to be referred to have a much wider scope.

For example, it will be remembered that last session in the Section of Pathology Dr. Bewley read an instructive paper upon the Pathology of Empyema, in which he showed

that empyema is always due to the entrance of pus-producing organisms into the pleural cavity, either directly from without, or through a diseased lung, or by the route of the blood and lymph-vessels.

Results such as these have been acquired by the patient labours of experts in modern microscopical research. While only the minority can be fellow-workers in this attractive field, the rich harvest which is being garnered can and ought to be eagerly seized upon by all practitioners who desire to keep pace with the rapid strides of knowledge.

It is scarcely necessary to add that I do not at all wish to convey that dermatology is becoming a mere sub-section of bacteriology. Rather does the skin afford an epitome of general and special pathology, and the attempts to unravel its problems have been fruitful in elucidating pathological questions.

I shall allude more particularly to instances wherein our pathological conceptions have been simplified, and our views cleared, and I hope to show that treatment has been rendered more rational and more successful.

If we glance at any of the current text-books upon diseases of the skin, we find a chapter or chapters devoted to the vegetable parasitic diseases of the skin. The affections usually described under this heading are some five or six in number.

Arranged in the order of the discovery of their exciting cause, we start from the important discovery made in 1839 by Schönlein, who was the first to recognise and accurately describe the fungous character of the elements constituting a favus crust. To this succeeded the discovery by Gruby and Malmsten in 1843 of the fungus of ringworm, and that of *tinca versicolor* by Eichstedt in 1846.

Notwithstanding the length of time these organisms have been known, and in spite of numerous investigations, their

exact botanical affinities and mutual relationships have not yet been satisfactorily determined.

Even in the case of favus, whose clinical characters are so sharply defined, observers are not agreed as to whether we should acknowledge one only or several distinct forms of fungus.^a

Many other problems await solution, some of them of old standing—viz., whether alopecia areata is a parasitic disease or not.

What a revolution is taking place in dermatology may be inferred from the fact that eleven years ago Kaposi, advertising to Hallier's mycological teaching, states that some of the investigations carried out in the direction indicated by him were so frivolous and resulted in such monstrosities that they excited the greatest mistrust of Hallier's results. "For," he scornfully adds, "there was no single disease, whether warts, eczema, psoriasis, pruritus (cutaneous or inflammatory), erysipelas, &c., which would not be attributed to a fungus" (Hebra, "Diseases of the Skin," New Sydenham Soc., Vol. V., p. 129).

Who, it may well be asked, would now deny that erysipelas and suppurative inflammations are due to parasitic organisms, to say nothing of the disputable ground presented by warts,^b eczema, and psoriasis.

When we consider the exposed position, the extent of surface, and the innumerable creases, folds, and crevices in the human epidermis, it is little matter for surprise that it has been found to be the home of a great variety of micro-organisms. Thus, Unna, in an investigation of sixty pure cultures from a series of cases of seborrhœic eczema, discovered no less than fifty different *Mucors*, twenty different kinds of

^a Cf. Pick und Král. Untersuchungen über den Favus. Monatsh. f. prakt. Dermat. XIII., p. 52.

^b Cf. Kühnemann. Monatshefte f. prakt. Dermatol. VIII., IX.

Penicillium, five Aspergilli, about a dozen forms belonging to the groups Oidium and Saccharomyces, besides a goodly number of partly known, partly unknown, cocci and bacilli.

Král succeeded in growing three pure cultures of mould fungi from "eczema marginatum," all of which possessed the common property of flourishing luxuriantly at the body temperature, while at the temperature of the room they grew but slowly and imperfectly (*Monatsh. f. prakt. Derm.*, 1890, p. 185).

In truth, there is an extensive Flora Dermatologica which vegetates upon the epidermis of man. The beginnings of our knowledge of it have been laid by Unna and his assistants, who have studied the appearances, macroscopic and microscopic, of these organisms, and their characters after cultivation in various media (*Monatsh. f. prakt. Derm.*, 1888, VII., p. 817, *et seq.*).

Many of these fungi are harmless, some are merely saprophytic, and a few are real mischief-makers (*cf.* Die Färbung der Mikro-organismen im Horngewebe. Unna. *Monatsh. f. prakt. Derm.*, XIII., p. 325).

As a matter of convenience, I have distributed the illustrations which I shall adduce under separate propositions:—

I. Some maladies hitherto ascribed to vague and unknown causes of internal origin are really traceable to infection, usually from without.

I shall upon this occasion dismiss with a nominal mention the case of lupus, whose specificity is generally acknowledged, and that of certain forms of purpura, which have been shown to be due to a fibrino-bacterial thrombosis (Tizzoni: Giovannini: de Gimard. *Monatsh. f. prakt. Derm.*, X., p. 473; XI., p. 74).^a

^a See also an important contribution by Kolb: zur Ätiologie der idiopathischen Blutflecken krankheit: *Arbeiten aus dem Kaiserl. Gesundheitsamte.* Bd. VII., 1891, s. 60.

Let us dwell shortly upon the examples of (a) erysipelas; (b) impetigo; (c) boils and carbuncles.

As to erysipelas, it may be regarded as proven that it is strictly caused by the intrusion of a streptococcus into the lymphatic channels of the skin or mucous membrane, and it is worth remarking that the contagious character of erysipelas was known in England long before it was recognised in Germany.

Erysipelas and lymphangitis are not convertible terms.

In connection with erysipelas, we are reminded of the interesting and important topic of the antagonism of micro-organisms, for erysipelas and vaccinia are examples of infective diseases, the artificial production of which has been suggested and used as a means of cure for other diseases—*i.e.*, lupus, and certain forms of tumour (Fehleisen, "Micro-parasites in Disease," Selected Essays, New Syd. Soc., p. 272; and Watson Cheyne, "Lectures on Suppuration, p. 83).

Now, erysipelas often starts from a source of suppuration, and may itself be complicated by or end in suppuration, and again, suppurative affections of the skin of primary and secondary origin are phenomena familiar to us in every-day practice.

It is generally held as settled that *acute* suppuration is, as Ogston first showed, due to the action of micro-organisms;^a but it is a difficult matter to determine whether erysipelas is an entity, a truly specific disease, or whether it does not represent a phase or mode of action of the pus-producing organisms. Watson Cheyne is inclined to uphold the specific character of the erysipelas germ; while Levy, in a valuable paper (*Archiv. f. exper. Path. u. Pharm.*, XXIX.

^a Karlinski gives interesting statistics, based upon 200 cases of purulent inflammation, of the different forms of cocci and bacilli concerned. *Monatsh. f. prakt. Derm.* X., p. 420.

Ueber die Mikro-organismen der Eiterung, p. 135), teaches that streptococcus pyogenes is at once an exciter of suppurative processes and of erysipelas. It is more than probable that there are several species of erysipelas due to different bacteria.

However this may be, it is a distinct advance in our knowledge to grasp the fact that the affections known as impetigo, boils, and carbuncles are invariably produced only under the influence of micro-organisms.^a We are thereby enabled to get clearer views of their clinical history and progress, and our treatment is rendered less haphazard and more scientific.

The clinical differences in these affections can be explained by such considerations as these. Pus probably varies in virulence according to its origin, and pathogenic micro-organisms certainly vary in virulence according to the external conditions in which they find themselves. In a word, the character of the mischief done—*i.e.*, the type of the disease—depends not alone upon its direct cause, but also largely upon the mode of entrance and the seat of development of the organisms (Bockhardt: Garré).

Levy (*loc. cit.*) adduces evidence to show that the *Bacterium coli commune* may induce all possible forms of inflammation—*viz.*, simple suppuration, inflammation of serous membranes, lymphangitis, and general blood-poisoning.

We are becoming more and more impressed with the conception, one of great importance, that the purely morphological study of bacteria is not the safe guide it was supposed to be in the dawn of bacteriology. A far more weighty point is the degree of virulence, coupled with investigations as to the conditions under which its virulence can be respectively augmented or attenuated.

^a Cf. Bockhardt's classical paper—Ueber die Ätiologie u. die Therapie der Impetigo, des Furunkels, u. der Sykosis. *Monatsh. f. prakt. Derm.* 1887. P. 450.

We can easily see why pustular eruptions are common on the heads of children infected with pediculi. The reason is that pyogenic organisms often lurk beneath the nails or exist attached to hairs, and hence are readily inoculated by scratching into the little wounds inflicted by the pediculi.

It is an old observation, and one which Mr. Hutchinson has emphasised in his teaching (*i.e.*, his aphoristic definition of *Impetigo contagiosa*—*viz.*, “common, contagious, curable”), that pus is contagious; but it should be remembered that pus, apart from the organisms which it contains, does not exert a pyogenic action (Cheyne). So that, strictly speaking, “*impetigo contagiosa*” does not represent a specific entity, because all forms of impetigo are, from their pathogenesis, contagious.

Considering the very great frequency of wounds and breaches of surface, it may appear strange that inflammatory and suppurative diseases are not more commonly met with. To this it may be replied that pyogenic organisms are not so abundant in the air as might be supposed, they are rarely present in putrefying fluids, and, moreover, they act only under certain special conditions. We are, besides, led by experience to conclude that susceptibility to the action of pyogenic organisms is, in many individuals, but slight. Virulence is probably altered by many slight and unexpected causes.

Is the dictum then true—“No bacteria, no pus?”

This is partly a verbal question, and turns upon our idea of what constitutes “true creamy pus” (Cheyne). Watson Cheyne leans to the doctrine of the essential connection between pus and bacteria (“Suppuration and Septic Diseases,” 1889, p. 29); while Burdon Sanderson considers it proved that suppuration can be induced without the aid of microphytes (Croonian Lectures, *Brit. Med. Journ.*, Nov. 14, 1891, p. 1033). V. Sehlen’s observations in Unna’s laboratory

confirm this latter view (*Monatsh. f. prakt. Derm.*, XI., p. 327).

If organisms which are usually pyogenic—viz., *Staphylococcus pyogenes*—may in certain cases be purely phlogogenous (Levy, *loc. cit.*, p. 162), why refuse to admit that non-microbic irritants may, under certain circumstances, excite suppurative inflammation? Can we determine when suppuration commences, and what test have we to decide when inflammatory exudation passes into purulent exudation? What is the “critical point” of suppuration?

Conversely it may fairly be asked, Does the presence of pyogenic organisms in an inflammatory effusion necessarily indicate that the effusion will become purulent? Levy (*Arch. f. exp. Path. u. Pharm.*, XXVII.) has shown that the presence of *Staphylococcus pyogenes* in a pleural effusion does not prove that it will become purulent.

At any rate, everyone admits that acute suppuration can be most surely brought about by micro-organisms.

Let us now for a moment stop to inquire in what ways these organisms gain access to the skin.

1. From without, through the epidermis. This is by far the most frequent mode. A wound or breach of surface offers an open door of entrance, but they may also enter through the uninjured epidermis, penetrating into the sweat ducts, hair follicles, and sebaceous gland openings. If the epidermis were an absolutely continuous horny sheet, devoid of glandular orifices, and remained uninjured, then we should undoubtedly seldom see a pustule or boil on the skin. Yet it must be admitted that pus cocci may penetrate into the lymph-channels of the skin through the intact epidermis (Garré). Bockhardt's and Garré's disinterested experiments upon their own bodies are conclusive as to the pathogeny of whitlow, carbuncle, and boils—viz., that they are local infective diseases. The well-known predilection of furuncles for

certain localities—*e.g.*, neck, loins, and axilla—is explained by the closely-applied parts of the dress favouring a thorough inunction of infective germs present over these parts, the result of this inunction being rendered correspondingly certain by the presence of the sweat (Garre, "Micro-parasites in Disease," Selected Essays, New Syd. Soc., 1886). If the micrococci invade only the epidermis we have a superficial pustule—*i.e.*, (*a*) impetigo^a; if the intruders find their way deeper down the hair-follicles and gland-ducts, we have a more violent inflammation, with or without necrosis—*i.e.*, (*b*) a boil, phlegmon, or suppurative folliculitis, and a congeries of furuncular points form (*c*) a carbuncle, which bears to a boil somewhat the same relation as that of a Peyer's patch to a solitary gland.

Ecthyma is scarcely worth retaining in nomenclature, for it is merely an aggravated form of impetigo, occurring chiefly on the lower limbs and on an unhealthy soil—*e.g.*, alcoholic or syphilitic.

2. The infection may occur from within outwards—*i.e.*, from the blood. This is true of only a small minority of cases. But it probably explains the occurrence of certain examples of mammary abscess, some of the instances of abscesses, boils, and other forms of local inflammation which are apt to occur, for example, in the wake of severe cases of continued fevers, epidemic influenza, &c. Pyogenic cocci have been found in the blood of patients suffering from various acute diseases, and by no means necessarily give rise to local troubles, for the production of which other concurrent factors are usually required. In eight cases of pyæmia and puerperal fever, and of which four died, Levy succeeded four times in demonstrating *Staphylococcus albus* in the blood of the living patients (Ueber die Mikro-organismen der Eiterung. *Archiv. f. exp. Path u. Pharm.*, XXIX., 136).

^a Hence, impetigo is not a mere stage or phase of eczema.

II. What have been commonly described as different forms or varieties of disease are often really due to the co-operation of different causes.

Let me give one or two illustrations. *Acne*.—Here we have two processes:—

(a) The closure of the follicle and the formation of a comedo.

(b) Suppuration within the follicle.

This arises either from the presence of micrococci which had been bottled up inside the plugged follicle or by subsequent infection. Hence, the two chief indications for local treatment are:—

(i.) To loosen and remove the horny plugs.

(ii.) To kill the micrococci which induce suppuration.

Scabies.—The eruption in this disease is described as consisting of papules, vesicles, and pustules. The pustules are really due to accidental secondary infection with pus cocci.

Syphilis.—Are pustular eruptions in a syphilitic patient the direct outcome of the syphilitic virus? Most of them are surely due to a “mixed infection;” and Leloir has since 1886 been in the habit of describing them in his clinique as purulent para-syphilitic complications.

Eczema.—The composite group of affections which we term eczema is being gradually disentangled.^a I can now mention only two points. First, the occurrence of post-eczematous boils, which sometimes occur as a complication of sequela. They are rare in connection with other diseases of the skin. We explain these boils and abscesses by inoculation of the skin with pus-germs, the pre-existing eczema furnishing a suitable soil. Secondly, the eczema impetiginosum (not to be confounded with primary impetigo con-

^a For example, the term “eczema marginatum” embraces several distinct affections—viz., (a) erythrasma; (b) tinea circinata; (c) tinea versicolor; (d) pityriasis circinata (Vidal); (e) eczema seborrhoicum; (f) forms of lichen circumscriptus (Vidal, Brocq). *Monatsh. f. prakt. Derm.*, XIII., p. 102.

tagiosa), common on the face of children, is not, as some authors maintain, a variety of eczema. It is really a hybrid condition—*i.e.*, an eczema upon which an impetigo has been grafted, by inoculation with staphylococcus. Hence, as we see, the disease is far more frequent on uncovered than on covered parts, because it is propagated by scratching. In the course of appropriate treatment, further, it is comparatively easy to stop the suppuration—*i.e.*, cure the impetigo—but we then still find ourselves face to face with a chronic eczema, with its notorious resistance to treatment (Dubreuilh. *Annal. de Derm. et de Syphl.*, 1890).

Lupus and Scrofuloderma.—Although the latter term is scarcely susceptible of exact definition, it is convenient to retain it for the group of affections characterised by these marks—*viz.*, indolent, dusky inflammation of the skin, readily undergoing suppuration, the tissue so spongy that it can be scraped away with the greatest facility, and leaving a thin white scar.

Such a condition of things is often conjoined with ordinary nodular lupus. In what relation to each other are we to view lupus and scrofuloderma? A plausible theory has been advanced by Unna, and Leistikow, who has examined the question bacteriologically, adopts the theory, which, briefly, is this, that scrofuloderma is an affection caused by the symbiosis of *Staphylococcus pyogenes aureus* and *Bacillus tuberculosis*. Or, in other words, scrofuloderma represents a case of “mixed infection” by the tubercle bacillus and by the ordinary excitors of pus.^a

According to the predominance of one or other of these disturbers of nutrition will the tissues exhibit a tubercular or a suppurative tendency.

^a Similarly, the gonococcus alone is unable to give rise to a suppurating bubo. When a suppurating bubo occurs as a complication of acute gonorrhoea it is always due to a “mixed infection” with pyogenic cocci, which are found in the pus from the bubo. The bubonic pus does not contain gonococcus.

The tubercle virus thrives best in organs which are readily prone to hyperæmia. The skin, which in most situations is anæmic, does not offer a very suitable soil for the bacillus. It fixes itself generally on parts which are normally hyperæmic—viz., cheeks, nose, and ears. Around a spreading lupus we always find a zone of capillary congestion.

Should the tubercle bacillus fail to meet with a persistent hyperæmia in its neighbourhood, then its deleterious action may be arrested, and it becomes encapsuled in a dry, caseous mass. Hence the extraordinary chronicity and the intermittent progress of the lupous disease. Pus cocci, on the contrary, always induce active hyperæmia and extravasation, wherever they penetrate into normal skin-tissue. But if the pus cocci arrive as secondary elements upon a tuberculised soil, they find there a badly nourished tissue, a soil unsuited for their development, and which does not allow them to produce acute phlegmonous inflammation. Instead, therefore, we get a gummy gelatinous softening of the tissue, a degeneration midway between the dry caseation of tubercle and purulent softening. Such a tissue would be more easily and more completely removed by “scraping,” and so, as clinical experience confirms, be less liable to relapse (Ueber Skrofuloderma; Dr. L. Leistikow.—*Monatsh. f. prakt. Derm.*, XI., p. 438).

Leloir holds views identical with these.

III. Classification of diseases of the skin is rendered more accurate.

One example will suffice—viz., sycosis. This term was for a long time, and is still, far too often used loosely in a merely topographical sense—*i.e.*, for any obstinate pustular affection involving the hairy parts of the face.

The next step in advance was the distribution of the cases into two groups—viz., parasitic sycosis (*tinea sycosis*), and non-parasitic sycosis (“ordinary sycosis” ! !). This sub-

division is maintained in our most recent text-books, notwithstanding Bockhardt's conclusive observations were published in 1887.^a

From the date of Bockhardt's paper we must deny the existence of a non-parasitic sycosis, and, in the present state of knowledge, the cases fall into three groups:—

- (a) Coccogenous sycosis; due to staphylococcus, the so-called non-parasitic sycosis of authors.
- (b) Hyphogenous sycosis; due to trichophyton, the so-called parasitic sycosis (ringworm of the beard) of authors. Exists as (i.) superficial, (ii.) deep form.
- (c) Bacillogenous sycosis; due to a bacillus named *Bacillus sycosiferus foetidus* (Tommasoli. *Monatsh. f. prakt. Derm.* VIII., p. 483).

(Cf. Ueber ulerythema sycosiforme. Dr. Sack.—*Monatsh. f. prakt. Derm.*, XIII., 133, and Disseminierte, parasitäre Perifolliculitis, *Ebenda*, XIII., 319).

IV. Since dermatology is at present in an active transitional state, the exposition of skin diseases in text-books will have to be largely re-cast. Much greater attention must in future be paid to ætiology.

V. We can better see the reasons for much of our management of skin affections, and we recognise more clearly the importance of prophylactic measures.

Ringworm.—Everyone knows the ease with which ringworm can be cured on the smooth parts of the body, or those only provided with lanugo, and its obstinate resistance to treatment when situated on the scalp. Scharff's observations (*Monatsh. f. prakt. Derm.*, X., 536) account for this. The human epidermis (horny layer) is a poor soil for the growth of trichophyton, which, in its turn, gives rise to only slight disturbances of nutrition in the skin, but even moderate inflammatory reaction is prejudicial to the life of the fungus,

^a Loc. cit.

which consequently perishes, or is easily cured by parasiticide and irritant applications. Scharff found mycelium very scantily present, and only in the deeper layers of the horny stratum and in the lower part of the hair follicle; none in the prickle layer.

Upon the head, on the contrary, the fungus flourishes luxuriantly, and produces, as a rule, little inflammatory reaction. The artificially produced irritation and inflammation, so often used in the treatment of ringworm of the scalp, finds then its prototype and its justification in the moderate reaction which occurs upon the smooth parts of the skin.

(*Cf.* An interesting series of bacteriological experiments upon the relative value of certain drugs in the treatment of favus and ringworm, by Dr. Schwengers.—*Monatsh. f. prakt. Derm.*, XI., 155.)

Erysipelas.—Accepting the parasitic nature of erysipelas as an undeniable fact, the position of those who, from clinical observation, have rejected the use of nitrate of silver and other local astringents in its treatment is amply justified. It is, I think, not more rational than the so-called abortive treatment of small-pox eruption; and, similarly, I must express my disbelief in the utility of the much-vaunted chalybeates internally.

Boils (Carbuncle) and Impetigo.—While the treatment of the superficial form of pustular dermatitis is often as satisfactory as it is simple, it is quite otherwise with the deeper grade of inflammation typified by a common boil. They are sometimes half welcomed—nay, if possible, encouraged by the laity under the lingering superstition that they afford an outlet for the evil spirits that torment the body, but ere long the patient will be ready to say:—

“Die ich rief, die Geister
Werd’ich nun nicht los.”

(Quoted by Garré.)

Now that the cause of boils is known to be parasitic infection, it will follow that, in the great majority of cases, they own an external origin, and the theory, still so dear to the minds of many, that they are entirely due to some vague constitutional cause ("impurity of the blood!") has received its quietus.

The indications for successful treatment fall naturally under four heads:—

1. To destroy by parasitocides the intruding cocci before they have induced necrosis of tissue.

2. When necrosis has occurred, to hasten as much as possible the expulsion of the necrotic plugs with their contained cocci.

3. To guard against the development of new boils through infection by means of cocci spreading from the older boils.

4. To place the organism in such a position as to render it as resistant as possible to invasion by micrococci.

As Veiel well points out (*Ueber die Therapie der Furunkulose*, *Monatsh. f. prakt. Derm.*, XI., 362), the first indication can seldom be fulfilled. For once the cocci have penetrated far enough to lead to visible swelling and formation of papule or vesicle, necrosis has, as a rule, begun, and the glandular ducts are clogged by exudation plugs, so that the external application of antiseptics frequently becomes futile. Abortion of a furuncle can sometimes be attained by first thoroughly cleansing the part and then applying Unna's carbolic-mercury plaster, or by injection with a 3 per cent. phenol solution.

For the second indication no method can, I think, approach in rapidity of cure the plan of thoroughly scooping out the necrotic core. Severe cases of pustular acne can be quickly and certainly cured only by following the same lines—*i.e.*, curetting or incising each pustule and carefully disinfecting its interior. This is very painful, and sometimes demands an

anæsthetic, but its results cannot be equalled. It is both rational and successful. Where this method is not available for boils, then, with Veiel, I believe in the old-fashioned poultice, which, at will, may be prepared with a $\frac{1}{1000}$ solution of sublimate or a 4 per cent. solution of boric acid. At night the boil may be dressed with the carbolic mercury plaster or with a boric acid paste (equal parts of vaseline and zinc oxide + 4 per cent. boric acid).

The third indication, which is an important one, is easily accomplished by thorough disinfection of the skin in the neighbourhood of the boils, especially over the seats of predilection, nape of neck, axilla, nates, &c.

In this mode a diabetic patient who, in nine months, had suffered from 160 boils was completely cured in seven weeks (Veiel). The greatest possible cleanliness must subsequently be observed as to habits, dress, and bedclothes, and the patient should keep his nails cut short and abstain from scratching.

Leloir gives a striking example which, perhaps, illustrates this point of contagiousness. A young man presented himself regularly every winter with a carbuncle or boil on his neck. At the first approach of cold he used to put on a great coat, the collar of which was dirty. This coat had belonged to his brother, who died of a carbuncle on the neck.

The fourth indication resolves itself into correcting any obvious derangement of health, and putting the patient under the best possible conditions of hygiene.

In the theory of causation we have to allow for factors other than the mere presence or accumulation of microbes in the blood and tissues—*e.g.*, depression of vitality, general or local, the existence of local inflammation, the influence of cold, injury, and individual predisposition; and, in regard to treatment, we must not overlook the influence of the state

of the blood—witness, diabetes and albuminuria—and the possible modifications of the soil that may be brought about by the state of the digestive organs, diet, and internal medicines (*cf.* Watson Cheyne, *loc. cit.*, p. 68); but upon these points it will, I imagine, be conceded by all that our knowledge is very defective.

One drug only may be mentioned.

I have no faith in the reputed virtues of calcium sulphide, in the prophylaxis or treatment of furunculosis, acne, and suppurating glands, and would place it side by side with that ludicrous specimen of therapeutic empiricism—viz., the treatment of amenorrhœa by pills of potassium permanganate.

DR. M'WEENEY cordially agreed with everything which Dr. Smith had said on the bacteriological side of skin diseases. His views on the subject of "mixed infection" were quite in accordance with those held by Babes and other Continental bacteriologists. Dr. Smith was to be congratulated on the use he had made of the facts that had been most recently established with regard to the ætiology of skin disease by Continental observers.

DR. FRAZER had tried extensively the sulphide of calcium in cases of boils and anthrax, and regretted to say it had failed to give any results such as writers would lead us to expect. Two different varieties of sycosis had been long familiar to him; the third he had never observed, which may be accounted for by its extreme rarity. It was satisfactory to find microscopic investigation explain the distinct features of these two affections, heretofore classed as sycosis, and usually confounded together.

DR. J. W. MOORE, in illustration of Dr. Smith's remarks as to the treatment of furuncles and anthrax by curetting, instanced the case of a lady, aged about 60, who suffered from anthrax of the cheek some eighteen months ago. Recognising the gravity of the attack, Dr. Moore recommended surgical treatment, and accordingly Dr. Charles Ball was called in consultation. He curetted the anthrax without delay, and the beneficial effects were simply magical—temperature and pulse-rate fell quickly, and a rapid and satisfactory convalescence followed.

DR. C. F. MOORE said he could corroborate what had fallen from Dr. Smith as to the effect of using clothes which had been in contact with disease germs.

DR. TWEEDY remarked, in corroboration of Dr. Smith's statement as to the antagonistic effect of erysipelas on lupus, that he had had under his care, some years ago, in Steevens' Hospital, a girl suffering from lupus of the nose, who, while under treatment, was seized with an intercurrent attack of erysipelas which resulted in the apparent cure of the lupus. The disease subsequently, however (four years after), reappeared.

DR. SMITH briefly replied.

AN ATTEMPT TO EXPLAIN THE EFFECT OF CLIMATE ON THE ACTION OF ANÆSTHETICS.

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[Read in the Section of Medicine, Nov. 20, 1891.]

ON reading the following in the first volume of the Reports of the Proceedings of the International Medical Congress of last year, I was led to consider that the effect of climate was not altogether explained by the greater readiness with which the chloroform vapour was dissipated in hot climates as compared with its condition in cooler latitudes.

Dr. Horatio C. Wood observes, at p. 140:—"A very curious parallel might be traced at this point between the experimental and clinical evidence in regard to the effect of climate upon the action of chloroform. In the Southern United States chloroform is used with great freedom, and with great alleged safety; and as long ago as 1878 Dr. Langdon B. Edwards, editor of the *Virginia Medical Monthly*, wrote—"It is one of the most peculiar facts I have ever known in medical practice, the difference of experience in Europe and the north, with chloroform and ether, as compared with that of the south, the high rate of mortality in the north and the low rate of the south. Further, in a recent letter to me, Sir Joseph Fayrer is very emphatic as to the safety of chloroform in India.'"

The very important and interesting paper of Dr. Wood has led me to bring forward this subject before the Royal Academy of Medicine, with a view to elicit opinion, and to ask those whose modern training, whose leisure and opportunities of research, and whose abilities fit them for a full and

searching inquiry into this—a matter of great interest and importance.

The point which struck me as bearing on the subject of this paper, in addition to those advanced by Dr. Wood, is that in hot climates the lungs play a comparatively less important part than they do in temperate and colder regions, and that, therefore, in the latter interference with their function, as in inhalation of chloroform, is more apt to be attended by serious consequences than in warmer latitudes, partly owing, as Dr. Wood advances, to the warm climate rendering the vapour of chloroform more easily diffused than at a lower temperature.

It is, I think, self-evident that the inhabitants of warmer regions are more independent of the action of their lungs than those of cooler latitudes, hence the greater endurance of the former in running and other like exercises; whereas the liver and skin seem to me to be of relatively greater importance in the persons of those who inhabit warm latitudes. This idea I do not regard as at variance with the views of Dr. Wood, Dr. Lauder Brunton, and others, and merely offer it as a suggestion in explanation as to the alleged greater safety in the use of chloroform in warm climates, in addition to that given by Dr. Wood of the greater diffusibility of chloroform vapour in the latter. Ether, on the other hand, being so much more readily diffusible, is not relatively so much affected by climate.

The *British Medical Journal*, Sept. 13, 1890, speaks of cases where the muscular walls of the heart and the hepatic cells were affected with fatty degeneration, from, as was considered by Professor Thierre, the somewhat prolonged use of chloroform—the patient in one case having been under its influence for more than one hour and a half. This man, though robust and temperate, and only thirty-six, was operated on for fractured patella by suture.

Animals kept under chloroform—especially when the dose was repeated for two or three days—were found to have the heart and liver changed in a similar way.

I introduce the foregoing to warn my hearers that although chloroform appears safer in general in warm latitudes, yet if its application be long continued, or often resorted to, another danger arises as just mentioned—a danger which we may infer to be more real and likely in warm climates than in those that are temperate or cold.

The Second Hyderabad Chloroform Commission^a reported as necessary to safety in chloroform administration, the recumbent position on the back, and perfect freedom of respiration, and strict and continuous attention to the condition of respiration as the most essential part of its administration—in fact, as far more necessary than observation of the state of the heart.

It would appear that such being the experience of able, practical, and scientific physicians in warm climates, those who use this potent agent in temperate and in cold regions should be doubly observant of these and other very full and important recommendations of the Commission, and of the very valuable observations of Dr. H. Wood, as published in the Proceedings of the Berlin Congress.

On referring to the work of Mr. Bryant, I find in corroboration of what I have quoted above from Dr. Wood's paper, that he says:—"In the more threatening cases commence instantly with artificial respiration, whether the respiration has failed alone, or the pulse and respiration together."^b

Dr. Wood^c shows the injurious effects of attempting to counteract the poisonous influence of chloroform or

^a *Lancet*. Vol. I., p. 158, &c. 1890.

^b Vol. II., p. 538. Second Edition.

^c Report of International Medical Congress, 1891. Vol. I., p. 146-7, &c.

ether inhalation by alcoholic or ethereal remedies however administered.

Digitalis, either before administration of chloroform, or after its effects have become injurious, he considers to have saved life in his experiments on dogs, and he "believes that in all cases of weak heart in man a full dose of digitalis before the administration of chloroform would greatly lessen the danger of cardiac collapse." Strychnine, and above all, artificial respiration, are what he has been led to regard the real remedies against death from chloroform.

The treatment of threatened danger from chloroform hardly comes within the scope of the present paper, as I originally intended it; but on the principle that some have held that the disease is to some extent to be known by the remedy, I thought it well to introduce some remarks and quotations on the subject.

As advanced by Dr. Wood, it has an importance in explanation of the greater safety of chloroform when used by inhalation in the tropics and other warmer latitudes than in the colder regions.

It is not out of place to advert to the influence which a warm climate, especially if it be also dry, as described by the late Sir James Clark,^a exercises "on almost all the functions of the animal economy, but more especially its obvious agency in promoting an equable distribution of the circulating fluids over the whole system, and particularly its effect in augmenting the activity of the circulation in the capillaries of the surface, and in diminishing in the same proportion the congestion of the internal vessels," and further on the same eminent authority continues—"If we take into account also the effect of the continual action of a bland atmosphere on the extensive surface of the respiratory organs, both as abating irritation of the lungs, and enabling them more

^a *Cyclopædia of Practical Medicine.* Vol. I., p. 419.

effectually to produce those changes in the blood that are essential to health, we have another very obvious explanation of the results observed."

It is hardly within the scope of this brief sketch to more than allude to what the late eminent Professor Casper^a termed chronic chloroform poisoning.

In such cases he held that hours, days, or even weeks may elapse, "during which the person anæsthetised remains continuously under the influence of the poison to which he at length succumbs." I may observe that most of the fatal cases investigated by Professor Casper occurred in cold weather.

The position which I believe to be borne out by the experience of those who have used chloroform in hot climates extensively, and which Dr. Wood puts forward, that it is a more safe anæsthetic when used there than when used in colder regions, is further borne out by what Dr. Wood records, and what I have myself ascertained, that the cat's reputed "nine lives" stands to it when subjected to chloroform inhalation.

Now the cat under almost all conditions lives, at least the domestic cat, in what approaches very nearly to a warm climate—I think everyone will admit this—much more so at all events than the average dog, at least such of the latter as are likely to be experimented on; and therefore this circumstance seems to bear out the position that I assume—to wit, that chloroform is more safe in hot climates than in cold, because the lungs, which are so seriously compromised in inhalation, are comparatively less important in high temperatures than in low. This fact is aided by the greater diffusibility of chloroform in the more elevated temperature, and the more active discharge of the functions of the skin and liver.

^a Casper's Forensic Medicine. Vol. II., p. 296. New Sydenham Society.

DR. FRAZER wished to say he had seen the first case of ether administration in Europe at the Richmond Hospital, and had given the first dose of chloroform at the same hospital to a patient of Mr. John Hamilton's. He had for a long time administered it at operations, and never had a single accident, which he attributed to giving his undivided attention to the exhibition of the anæsthetic. He had, however, in private practice met two cases of serious symptoms, both fortunately ending favourably; these were not due to the anæsthetic but to syncope occurring in various individuals as the effects of the anæsthetic passed off. He had to direct attention to the fact that some persons eliminated the anæsthetic more slowly than others; in one case, for example, the vapour of ether continued to pass off for at least three days. Under the late Sir D. Corrigan he had given it in many cases of *delirium tremens*, and the result was such that the practice was abandoned, serious symptoms invariably occurring, but none ending fatally. As for continued administration in a case of tetanus, he had aided in using it for upwards of two days; the chloroform gave relief, and enabled the patient to take food, but the disease progressed and ended fatally.

DR. MOORE, in reply, agreed with Dr. Frazer's remarks as adding to the interest in the subject, and stated that he was well aware that it was only in exceptional circumstances, as in Calcutta, that ether could be had in a warm climate.

SOME RECENT AIDS TO THE DIAGNOSIS AND TREATMENT OF DISEASES OF THE STOMACH.

BY H. C. TWEEDY, M.D., F.R.C.P.;

Physician to Steevens' Hospital.

[Read in the Section of Medicine, December 18, 1891.]

AT the commencement of last year's Session this Academy had the privilege of hearing from Professor Purser a dissertation on "The Modern Diagnosis of Diseases of the Stomach," which will long be remembered by all of us who were present on that occasion.

The absolute clearness with which this complex subject was treated, and the closeness of reasoning displayed, following each point to its logical conclusion, were sufficient to account for the lasting impression left upon our minds, while the labour and research evidenced in every page render the lecture, now fortunately preserved for us in the Transactions of the Academy, a perfect storehouse of information for all who may wish to refer to the extensive literature of diseases of the stomach.

Dr. Purser dwelt chiefly on the modifications the digestive process, as carried on in the stomach, undergoes in disease. In this paper I shall endeavour to lay before you very briefly some mechanical aids now at our disposal for the physical examination of the organ itself, for obtaining portions of its contents for chemical analysis, and for the treatment of some of its most common diseases.

I shall submit to your consideration two methods of physical examination which, as far as I am aware, are not employed here as frequently as they should be. Also two

recent appliances—one for obtaining small quantities of stomach-contents for diagnostic purposes, and the other for applying electricity directly to the stomach.

The first mode of physical examination to which I would draw your attention is what has been termed the “*Splashing Sound*,” or “*Clapotement*”—a method of examination much lauded by Riegel, and Obraztsoff, and also by Boas,^a who states that he finds it gives better results than percussion.

This sound may be elicited by pressing repeatedly with the tops of the fingers in the epigastric region. Its lower limit rarely extends below the level of the umbilicus, unless the stomach be dilated, or else displaced downwards. It may be heard in healthy persons more or less plainly if they have taken a large quantity of fluid, or if the abdominal walls are relaxed and not overloaded with fat.

This method has a double utility. In the first place, we may be able to ascertain by it the size and position of the stomach. If, for instance, we palpate very gently from below upward till splashing is perceptible, then we may in many cases be able to arrive at a fairly accurate conclusion as to the contour of the stomach; and not only so, but we may also get an idea as to the condition of its muscular layer, for let a healthy person drink 50 or 100 grammes of water we can detect no splashing, even when the abdominal walls are thin; on the other hand, if the muscular layer of the stomach be wanting in tone (although there may be no dilatation or retention of the food taken) the sound may be produced very distinctly by the above-mentioned quantity of water, or even by a smaller quantity.

Under some circumstances splashing may originate in the transverse colon; but in this case the splashing is

^a *Allgemeine Diagnostik und Therapie der Magenkrankheiten.* Leipzig. 1890.

found along a straight line, or along a curve which is slightly convex *above*, and may thus be distinguished from that originating in the stomach by the fact that the latter forms a convex line *below*, and ascends distinctly from the median line. Should these differences not be distinctly marked, we may inflate the stomach by means of a tube and double-rubber bulb. After insufflation of air the splashing sound in the stomach ceases, while that in the colon persists; but as soon as the air is allowed to escape from the stomach the splashing sound immediately reappears.

This artificial distension of the stomach by gas or air has been much employed on the Continent as an aid to diagnosis, with the object of enabling us to map out the contour of the organ, especially the greater curvature, in a more accurate manner. It was originally introduced by Frerichs and Mannkopf, who carried out the process by generating CO_2 by the separate introduction of solution of tartaric acid and bicarbonate of sodium. Boas and others accomplish the same object, gradually inflating the stomach by means of the double-rubber bulb, attached to an ordinary stomach tube. This method, which was first employed by Runeberg, possesses striking advantages over insufflation by CO_2 .

In the first place the quantity of air employed can be accurately gauged and controlled so as to increase or diminish it according to circumstances. Moreover, the estimation of the amount of air employed is of itself useful for diagnostic purposes, as a relaxed and dilated stomach requires much larger quantities of air than a healthy stomach with normal muscular tone. The outlines of the stomach are also brought into greater prominence, so that the portion of it lying against the abdominal wall can, as a rule, be palpated thoroughly and without undue haste.

whereas in the carbonic acid method this can only be done while the development of the gas is taking place; for as soon as this process is completed the CO_2 escapes rapidly from above or below, and the stomach again contracts before a thorough examination can be made.

During artificial distension of the stomach with CO_2 or air, it sometimes happens that the stomach itself does not become enlarged, but that the outlines of the intestines become unduly prominent. This condition was first observed by Ebstein, and described as insufficiency of the pylorus. Boas and Ewald have demonstrated, however, that these cases are accompanied by an abnormally rapid faecal evacuation, and both writers are of opinion that, aside from insufficiency of the pylorus, there is in such cases an accelerated discharge into the intestines of alimentary substances undigested in the stomach.

Numerous methods have been suggested with the object of demonstrating the size, position, and capacity of the stomach; notably those of Schreiber, Rosenbach, Fleischer, Jaworski, and others, all of which may be found fully described in Boas' recent work. Also the attempts at electric illumination of the stomach by gastrodiaophany, as lately proposed by Dr. Einhorn, by means of a Nélaton's tube, provided with an Edison's incandescent lamp, on the plan of Voltolini's method of illuminating the larynx.

These methods, however curious and interesting, are practically useless now that we possess in insufflation a procedure which is both certain and devoid of danger when practised under proper precautions. Boas lays down the following contra-indications to the use of air or CO_2 , as well as to the introduction of the sound:—(a.) Constitutional or local diseases in which the irritation connected with use of the sound, or insufflation, may enhance the disease or threaten the patient's life; (b.) Gastric and intestinal

diseases, which can be diagnosed without resort to the sound, &c.—especially gastric ulcer and adhesions of the stomach-walls, and also suspected atrophy of the gastric mucous membrane.

There is one purpose for which up till very recently the use of the stomach-tube has been regarded as indispensable—viz., the obtaining of portions of the contents of the stomach for chemical analysis by Ewald's^a "expression method." There are sometimes, however, difficulties in this mode of examination which, all-important as it undoubtedly is, render both doctor and patient too ready to dispense with it, often to their mutual injury.

Several contrivances have been suggested as alternatives for the use of the tube. Edinger and Späth^b suggested that the patient should be made to swallow particles of elder-pith, stained with appropriate re-agents, or leaden balls, to which are attached threads stained with the test re-agent.

Edinger^c also suggested the plan of causing the patient to swallow a small sponge attached to a thread. The sponge was allowed to remain in the stomach for several minutes, and then withdrawn, after which the contents were pressed out and examined for HCl. This test, however, fails in two particulars—first, the sponge is partly expressed during its withdrawal through the œsophagus, and thus loses much of its contents; secondly, it absorbs fluid from the moisture of the œsophagus and pharynx—thus, the stomach-contents cannot be examined pure.

With a view to obviating these disadvantages, Dr. Einhorn,^d of New York, has devised an ingenious little

^a Ewald and Boas. *Virchow's Archiv.* CI., 325, 1886, and CIV., 271, 1888.

^b *Münich. med. Wochenschrift.* XXXIV., 51. 1887.

^c Edinger. *Deutsche Archiv. f. klin. med.* Bd. 29, p. 555.

^d *N. Y. Med. Rec.* July 19. 1890.

instrument, which he terms a "stomach-bucket," and of which I have the pleasure of exhibiting to you this drawing. This consists of a small oval vessel— $1\frac{3}{4}$ cm. in length, $\frac{3}{4}$ cm. in breadth—made of silver; the upper portion is open, but bridged over by an arch of the same metal, to which is attached a silk thread.



Dr. Einhorn gives the following directions for the use of this apparatus:—

"The patient is asked to open his mouth widely, and the little vessel is placed on the root of the tongue (almost in the pharynx), after which the patient is to swallow *once*. The vessel reaches the stomach in a short time (one-half minute to one and a half minutes). This point can be easily found by the length of the thread from the vessel to the mouth; it equals the distance from the teeth to the cardia, which is usually 40 cm. It is advisable to make a knot on the thread, marking 40 cm. When this knot comes into the mouth, then we are sure the vessel is in the stomach. The vessel having been left in the stomach for about five minutes, is then withdrawn. During withdrawal of the apparatus, a resistance is felt at the introitus oesophagi. To overcome this difficulty, when the apparatus is at that narrow point the patient should either deeply expire or swallow once. By the act of swallowing, the larynx is pushed upward and forward, and thus the passage is free.

"If the stomach is not empty the vessel returns filled, and the amount is sufficient for making qualitative tests for free hydrochloric acid and the rennet ferment."

"In people suffering from an abundant secretion from the mucous membrane it may happen that the bucket may become filled with mucus before entering the stomach. (In emptying the stomach it is always easy to distinguish real stomach-contents from plain mucus.) In case we find principally mucus in the vessel, it is necessary to make the trial again, and to cover the opening with a thin gelatinous capsule, which keeps the mucus away from the vessels on its passage to the stomach; there the capsule is dissolved, and the stomach-contents can enter unmixed into the apparatus. On its return from the stomach, the 'bucket' being filled, the mucus cannot to any extent enter into it."

"If the sample from the 'stomach-bucket' give a positive reaction of HCl (with Congo, or Günzburg's phloroglucin-vanillin test^a) then the same is made use of at once; but should it prove negative as to the presence of HCl the obtained sample must be examined more minutely, and if it is found to be mixed with much mucus, then it is necessary to obtain another sample with capsule covering as described above."

The best time for making an examination is just as usual—one hour after Ewald's "trial breakfast,"^b or three to four hours after a trial dinner.

It goes without saying that for a full and exact analysis of the contents of the stomach the use of the tube is indispensable, but Dr. Einhorn claims certain advantages for his apparatus, which I can fully endorse:—

1st. It is more simple and easy to use than the tube, and causes no exertion to the patient.

2nd. It is a great advantage to the general practitioner, who does not intend to make an exact analysis of the

^a *Ctbl. für klin. Med.* 1887. P. 737.

^b For the relative value of the "Probefröstück" and "Probemittagbrod," see the *Berlin klin. Wochenschr.* 1888. No. 32, 647.

stomach-contents, to be able in this rapid manner to determine whether there exist free HCl or not.

3rd. Even in gastric ulcer there is no danger whatever from a hæmorrhage in consequence of the examination, for which reason this method may safely be employed in cases where there is a suspicion of an ulcer of the stomach, and where there may be danger in using a tube.

4th. The introduction of the bucket may elicit information regarding several other points connected with the œsophagus and stomach, *e.g.* :—

(a.) The permeability of the œsophagus.

(b.) In extracting the vessel, the determination whether the cardia is closed (in this case a certain resistance is felt as soon as the “bucket” touches the cardiac orifice).

(c.) One is enabled, as soon as the bucket is in the stomach, to study to some extent the contractions of the organs by observing how far, with what force, and at what intervals the thread is pulled further in, for the thread *alone* affords too few supporting points to be moved by the contractions of the œsophagus. In this way every traction of the thread hints at further locomotion of the apparatus in the stomach.

Let me now say a few words regarding the use of electricity in the treatment of diseases of the stomach. It has long been known from the experiments of Ludwig and Weber, ^a von Ziemssen ^b and Bocci,^c that galvanic and faradic currents, applied directly to the stomach of animals, produce contractions of the organ, and also an increased secretion of gastric juice. But for years past currents

^a Kussmaul. Arch. f. Psych. und Nerv. 1877. Volume VIII., p. 205.

^b Von Ziemssen. Klin. Vorträge. No. 12. Die Electricität in der Medicin.

^c Bocci. Lo Sperimentale. June, 1881.

have been applied percutaneously over the human stomach by a number of observers—notably, Neftel,^a Fürstner,^b Leube,^c Richter,^d and others—with beneficial results in dilatation resulting from chronic catarrh, in nervous dyspepsia, and other diseases; and more recently Ewald and Einhorn^e demonstrated an increased mobility of the stomach under the influence of percutaneous electricity, by the appearance of the salol test^f in the urine about one-fourth of an hour earlier than usual; and A. Hoffmann^g showed that the galvanic current, applied in the gastric region for twenty minutes, produces an abundant secretion of gastric juice.

Notwithstanding all this, it was considered very doubtful if electricity applied in this manner was able to produce to the full its beneficial effects, as it was considered questionable whether any direct peristalsis results, but rather (as held by Kussmaul,^h Pepper,ⁱ and others), that the good results obtained in cases of dilatation were mainly attributable to the favourable influence of the contraction of the abdominal walls. Accordingly, Duchenne^j and Kussmaul,^k and later, Bardet,^l Baraduc,^m Stockton,ⁿ Ewald,^o and Boas^p have recommended direct electrification of the stomach, using

^a Centralbl. f. d. med. Wissensch. 1876. No. 21, p. 370.

^b Berl. klin. Wochenschr. 1876. No. 11.

^c Deutsch. Arch. f. klin. Med. 1879. Tome 23, p. 98.

^d Berl. klin. Wochenschr. 1882. Nos. 13 and 14.

^e Verhandl. d. Vereins f. innere Med. 1888. P. 58.

^f Ewald and Seivers. Klinik d. Verdauungskrankheiten. II., 53.

^g Berl. klin. Wochenschr. 1889. Nos. 12 and 13.

^h Arch. f. Psych. und Nerv. 1877. VIII., p. 305.

ⁱ Philad. Med. Times. May, 1871. P. 274.

^j Cited from Kussmaul.

^k Loc. cit.

^l Bull. Gén. de Thérap. 1884. Tome 106, p. 529.

^m Journ. de Med. Practitioner. Dec., 1888. P. 455.

ⁿ N. Y. Med. Rec. Nov. 9, 1889. P. 530. And Amer. Jour. Med. Sc July, 1890. P. 20.

^o Klinik d. Verdauungskrankheiten. P. 64. Berl. 1889.

^p Allgemeine Diagnostik der Magenkrankheiten. P. 239. Leipzig. 1890.

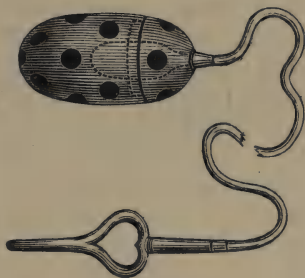
various electrodes which were in fact all modifications of the stomach-tube, constructed so as to act as an insulator to the wire. The evident inconvenience of this bulky arrangement and the fact that the tube containing the electrode had to be kept in the throat during the whole time the electricity was being applied, has prevented this method of treatment from attaining any great popularity, as only persons accustomed to the frequent use of the stomach-tube could bear the procedure at all, and even to them it causes considerable discomfort.

This important mode of treatment is, however, likely to receive a fresh impetus, in consequence of the recent production by Dr. Max Einhorn^a of a new electrode, by which the disadvantages alluded to have been obviated. It is constructed on the same principle as the "stomach-bucket" which has just been described, and once swallowed reaches the stomach without further artificial aid. The silk thread of the bucket is represented in the electrode by a very fine (1 mm. in diameter) rubber tube, through which a very fine, soft, conducting wire runs to the battery. The end-piece of the electrode consists of a hard rubber capsule of the same size and shape as the "stomach-bucket," with many openings. In this capsule lies a metallic button, which is connected with the wire. The rubber capsule serves to avoid direct contact of the metal with the stomach-walls. The circuit is completed in the same way as in Bardet's electrode, by the water the stomach contains. The other (ordinary) electrode is best placed either on the back to the left of the seventh dorsal vertebra, or in front over the epigastric region, or is simply held in the hand.

The electrode is introduced and withdrawn in the same manner as the "stomach-bucket." No force is necessary

^a N. Y. Med. Rec. May 9, 1891.

for its withdrawal—one has only to make the patient swallow once or twice, and to make use of the moment when the larynx by this act ascends, and the passage becomes free to withdraw the electrode, which is now done with perfect ease.



Dr. Einhorn further describes a series of test experiments made upon healthy persons, with the result that, in each case, after the application of direct faradism there was a marked increase in acidity. The conclusion that the faradic currents increase the production of the gastric juice seems to be justifiable.

As regards clinical experience, the most marked results have been obtained in cases of dilatation, and also in chronic gastric catarrh; the improvement in the latter cases being demonstrable by chemical analysis. Two cases especially are to be noted of very aggravated chronic gastric catarrh in which free HCl could never be found after the test meals; the same could be easily detected after the stomach had been faradised internally for ten minutes. Dr. Einhorn also mentions two cases of pure gastralgia, which derived benefit from the internal application of the galvanic current.

I cannot pretend to have offered to you in this paper anything original, or even anything particularly novel; but I was desirous of bringing before the Section, however

imperfectly, a subject which, in the course of some reading and a little observation, had been of particular interest to myself.

MR. GEORGE FOY said that the disadvantages of Einhorn's bucket were—(1) It may be stopped by disease of the œsophagus and prevented from entering the stomach; (2) its contents may have been got in the œsophagus; (3) it will probably empty itself in the mouth; (4) it is inferior to the Abbé Spallanzini's metal capsules; and being modelled on the Abbé's capsule, Einhorn is not entitled to credit for its introduction.

DR. M. A. BOYD considered that the splash symptom mentioned by Dr. Tweedy was of all symptoms the most reliable in all forms of dilatation of the stomach, both functional and organic, and that it is a common symptom in a great many of the catarrhs of the stomach which are known as dyspepsias. Functional dilatation accompanied by this symptom is a very common affection, and will be found if looked for in most cases of anæmia or chlorosis, with the characteristic left-side pain, even where no gastric ulceration is present, and also where the ingesta of injudicious food and much fluids lead to fermentation and distension of the muscle-wall of the stomach. Distension of the stomach from an atonic condition of its muscle, is not uncommon after long illnesses. In all these cases the splash symptom is present. In organic stricture of the pylorus from malignant disease, contracting gastric ulcer, or fibroid induration, the most characteristic symptoms of dilatation are met with, and the splash symptom is most easily obtained. When dilated from organic stricture, all portions of the stomach are stretched. In functional dilatation the only part distended is the cardiac end where the muscular tissue is most abundant, showing that muscular atony in addition to the weight of fluid ingesta are the most important factors in its causation. He regarded the splash as always indicative of some abnormal condition of the stomach.

DR. TWEEDY remarked in reply—first to Mr. Foy, that however valuable might have been the discovery of the Abbé Spallanzini, 200 years ago, in obtaining by a sponge the contents of the stomach, the contents so obtained would have been of little service to him, as the diagnostic value of chemical examinations dated

practically within the last ten years. He added that from personal experience he could state that the introduction of Dr. Einhorn's stomach bucket was attended with little or no inconvenience ; and that it was not likely to empty its contents on withdrawal from the stomach, both in consequence of the consistence of the alimentary matters withdrawn, and of the small size of the opening in the bucket ; but even if some of the contents escaped, one drop was sufficient for the performance of the test for hydrochloric acid with Günzburg's phloroglucin-vanillin solution. Another objection mentioned was anticipated by Dr. Einhorn, in providing a gelatine capsule for the stomach bucket, where there was a superabundance of œsophageal mucus. In the descent to the stomach no mucus could enter the bucket in consequence of the gelatine covering, and as the bucket is full when withdrawn it is impossible that any amount of mucus could displace the stomach-contents already in the instrument.

ACUTE DOUBLE PNEUMONIA SUCCESSFULLY TREATED BY BLEEDING AND INHALATION OF OXYGEN.

By GEORGE FOY, F.R.C.S.;

Surgeon to the Whitworth Hospital, Drumcondra.

[Read in the Section of Medicine, December 18, 1891.]

THE case I bring under notice is that of a man, twenty-four years of age, who was admitted into the Whitworth Hospital, Drumcondra, on the 6th of June, 1891, suffering from pneumonia.

The disease extended over the whole of the left lung, and the posterior lobe of the right.

When seen the temperature was found to be $104\cdot6^{\circ}$, the pulse 112, and the respirations 36. A quinine and acid mixture was prescribed, and some pounded ice; this latter he took greedily and enjoyed greatly.

On the morning of the 9th of June he was so ill that death seemed impending; his face was almost purple, and his heart was acting slowly and laboriously, so that its stoppage seemed near at hand. Something had to be promptly done to relieve venous congestion, and I opened the median cephalic vein and let blood to sixteen ounces. Immediately on the vein being cut a jet of ink-black blood shot some six feet off, and when the stream was caught in a vessel and kept for days it retained its dark colour, and neither buffed nor cupped. Before the bleeding was stopped the patient expressed the greatest relief, and his face lost its dusky hue; the pulse became soft and compressible, and the patient fell into a sound sleep, which lasted for six hours.

Such good results were very satisfactory, but they were not permanent; in two days all the good effects were gone, and the old symptoms of venous congestion returned, and the disease, now in its eighth day, was pursuing an unfavourable course. Ammonia was prescribed, and a plentiful allowance of Valentine's meat juice ordered. Towards evening of this day—his fifth in hospital—respirations were 64 in the minute, and he was unable to hawk up the sputum; the temperature had fallen to 103° , but I did not think the fall of temperature betokened other than danger.

He commenced to wander in his mind and pick at the bed-clothes. The greater part of his trouble was, I concluded, due to interference with the oxygenation of his blood, and as three-fourths of his respiratory space was functionally useless, I saw no way of oxidising his blood other than by increasing the amount in the air he breathed. By the aid of Mr. Turner, of Messrs. Fannin, of this city, I got a suitable apparatus, and on the evening of the 11th the patient commenced the inhalation of oxygen gas, which continued from 7 p.m. to 7 15 p.m. Gradually all the lividity of the face disappeared, and consciousness returned. On the inhalation being discontinued he slept for an hour.

Although the oxygen gas was driven directly into the mouth-piece, which was placed between the patient's molar teeth, no excitement but quiet followed its administration. At 10 30 p.m. the inhalation was repeated, and again during the night. For four days oxygen was administered every three hours for 15 minutes at a time.

On the morning of the 12th, at 10 a.m., the temperature was 100.8° , respirations 40, and the pulse 106. He expelled the sputum without difficulty, and was able to answer questions, but not to make any long statement. Oxygen was continued until all the more urgent symptoms disappeared; 33 feet in all having been given. Resolution set in and

proceeded but slowly, and it was not until the 24th of August that the patient was discharged.

During convalescence the patient told me that on the night of the 2nd of June he travelled from Liverpool to Dublin on the deck of a cattle boat. He was thinly clad, and when the vessel was about half-way he got a severe shivering fit, and on reaching Dublin he went straight to his lodgings and got into bed; a warm jar was placed to his feet, extra clothes put on the bed, and a fire lit in the room, nevertheless he felt "stone cold," and shivered in bed. Warm drinks were given to him, and he got rid of the shivers, but got a violent headache, and during the night his landlady declared he was delirious. He was better the following day, but became again delirious at night, and remembers nothing more until he found himself in hospital.

My object in bringing the case forward is to call attention to two old-fashioned methods of treatment—bleeding and inhalation of oxygen.

Perhaps no therapeutic remedy ever had so much talent given to the investigation of its action as oxygen. Leonardo da Vinci, in his treatise on "Air and Flame," published about 1480, describes the necessity for the gas and tells of its anti-putrescent power. Mayow, 200 years later, performed his well-known experiment on a mouse with his "nitro-ærial" spirit, stating "that the igno-ærial particles absorbed in respiration are designed to change venous into arterial blood." His experiments were, in 1774, repeated by Lavoisier and by his pupil, Beddoes, the father of "Pneumatic Medicine." Priestley also experimented with his dephlogisticated air, and was so delighted with the results that he entertained the idea of purifying public assembly rooms by means of his newly-discovered gas. Fontana and that able physiologist, the Abbé Spallanzani, all became interested in the therapeutic possibilities of "nitro-ærial"

or dephlogisticated gas. Ingen-Houzen, in 1782, found that after the inhalation of oxygen his sleep was sweeter and more refreshing than usual. Poulle, in 1785, laid down certain directions for its use, and describes the conditions calling for the use of the "Vital Air," in which treatise, which was published in Latin at Montpellier, he recommends the inhalation of the gas to "those asthmatics whose condition is not caused by excess of irritability;" and further, "it is the grand resource in restoring to life asphyxiated persons. Finally, it will prolong the last moments of the aged by rekindling in them the fire of life about to be extinguished." In a prize essay submitted to the Medical Society of Paris, in the Year of Liberty, on the therapeutic value of "Vital Air," he records the benefits a lady of thirty-one years of age, suffering from consumption, derived from its use.

Time does not allow of a chronological statement of the many medicinal uses to which oxygen gas was applied; but I may mention that Dumas, in 1792, declared that its inhalation in phthisis was baneful, and performed some experiments on dogs which appear to support his views. "I think I can set forth," said he, "founded on experiments, that the continued use of such an air would start in the lungs that degree of lively irritation which sometimes leads to a tuberculous or ulcerous state, of which that disease is the necessary consequence." We should not, however, forget that Dumas in his experiments used oxygen obtained from oxide of mercury, the impurity and danger of which was pointed out by Priestley. Beddoes adopted the views of Dumas on the action of oxygen in phthisis, and he treated the disease with sub-oxygenated air, and he supported his theory by the statement that pregnancy arrested phthisis; and he asserted that the blood of pregnant women is deficient in oxygen. Reasoning from these grounds, Beddoes, in his pneumatic

hospital at Clifton, confined phthisical patients to rooms in which, by charging the chamber with hydrogen gas, he produced an atmosphere deficient in oxygen. The Hotwells Hospital became famous for its treatment of palsy, epilepsy, asthma, chlorosis, hysteria, typhus, dyspepsia, and even leprosy, by the pneumatic process.

The staff was enthusiastic in praise of the method, and their labours were apportioned thus:—Mr. Watts attended to the apparatus; Mr. Davy prepared the factitious airs; and Dr. Beddoes took charge of the patients. The patients were directed to take “the point of the faucet between the lips, inhaling from the bag through the mouth, and to expel the air from the lungs through the nostrils, which operation most patients readily acquire a habit of performing with ease.” The reputation of factitious airs in almost every form of disease was heightened by the publication by Dr. Beddoes of letters from Drs. Thornton, Cross, Barr, Carmichael, Pearson, and others, all lauding the new remedy; and in 1795 Mr. James Watt published a tract on the subject.

The first use of oxygen gas in this country of which I can find mention is that of Dr. R. Reid, who read a paper before the Association of the Fellows and Licentiates of the King and Queen’s College of Physicians in Ireland on the 7th of July, 1817, entitled “The Use of Oxygen Gas in Angina Pectoris,” which appears in the first volume of the Transactions of the Association.

Gradually the remedy fell into disfavour; probably the impurities that were present in all specimens of the gas prior to its being prepared from atmospheric air had much to do with its being laid aside. Within the last few years, however, the gas is coming to be largely used as a therapeutic agent of undoubted value; and since MM. Brin have succeeded in producing a perfectly pure oxygen the remedy

can be easily tested and its merits decided. Cobell, in 1874, published a series of interesting and valuable articles on oxygen as a remedy in disease, in the *Virginia Medical Monthly*. And Dr. Goolden, of St. Thomas's Hospital (*Lancet*, Oct. 25th, 1879), found that oxygen gas promoted the healing of a sloughing ulcer of the throat. Dr. Francisco Valenzuela, of Madrid, in a paper before the Academia Médico-Quirúrgica, in May, 1887, stated, as the result of a number of experiments made by him on rabbits to determine the effects of oxygen of high tension on the animal organisation, that he found it invariably to lower pyrexial temperature.

The Council of Municipal Hygiene of Paris, in June, 1888, directed Dr. Voisin to draw up a code of instructions for the administration of oxygen gas to the asphyxiated. Not long since the *Medical Press and Circular* reported the successful administration of the gas to a man who was asphyxiated from carburetted hydrogen gas.

In my book on Anæsthetics, I have recommended the gas as a remedy in anæsthetic narcosis. The therapeutic use of the gas is being tried in every country and for the great majority of diseases; but its value in pneumonia seems not to have attracted much attention, as may be seen by consulting Neale's Digest. Dr. John Chambers has, in the *Lancet*, reported favourably of its use in a severe case of pneumonia—indeed he ascribes the patient's recovery to its use; and his is the only British case I can find a report of.

Abroad it has been generally used for pulmonary diseases. Amongst the warmest advocates for its use are Wallian, of New York, whose numerous papers on the subject are summarised in Sajous' Universal Medical Annuals for 1888, 1889, 1890, and 1891, and Dr. W. G. Thompson, whose paper is noticed in Cassell's Year-Book for 1890, and in Sajous' great work.

No matter what theory we may accept of the pathology of pneumonia, I think we cannot go far astray in employing Nature's antiseptic—an innoxious agent, and the one of all others most suited for the aeration of the blood and the oxidising of the broken-down tissues, and so facilitating their elimination by the kidneys. And not the least of its benefits is that of giving sleep to the worn-out patient—natural, refreshing sleep.

DR. M. A. BOYD said he was glad to hear attention called by Mr. Foy to a remedy in pulmonary congestion which we did not try in such cases as often as we might. He (Dr. Boyd) had only a limited experience of oxygen in lung cases, but he had tried it and considered it a most valuable remedy in disease of the heart with degeneration. He brought forward during last Session of the Academy a case of Cheyne-Stokes' respiration with degeneration of the heart, where the improvement following the inhalation of oxygen was most remarkable, the degenerated heart strengthening and the dyspnœa disappearing after it was used for some time.

DR. C. F. MOORE and the CHAIRMAN having also spoken,

MR. FOY, in reply, drew attention to the discussion in the past as to the side on which pneumonic patients should be bled—Sydenham's love of bleeding in pneumonia, and Cullen's classification of bleeding—the use of oxygen in Dublin at the beginning of the century, and Dr. Reid's paper on the value of the gas in 1817—the case of General Philip Sheridan.

SOME RECENT MODIFICATIONS IN OUR VIEWS OF ENTERIC FEVER AND ITS TREATMENT.

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[Read in the Section of Medicine, December 18, 1891.]

SINCE Murchison wrote his classic treatise on Enteric Fever we have added almost nothing clinically to the accurate and scientific description he gave of the disease. He covered in that description almost the entire ground in connection with the disease and its history, and in the closeness of his reasoning as to its ætiology he foreshadowed all that bacteriology has since discovered regarding it. In fact, it is only in its bacteriological aspect that any additions have been made to the subject, and it is from this side alone that any additional knowledge is likely to come which can finally determine the mode of its origin. The additional bacteriological data, however, we now possess have considerably narrowed the issues, and enabled us to regard the disease from the standpoint of being an acute infective disease, which heretofore it was not considered, in which, like phthisis and pneumonia, a special bacillus plays the important part.

Since that bacillus was discovered by Koch and Eberth, and its peculiarities and mode of growth were studied, more especially by the latter observer, our views as to the ætiology of the disease have considerably changed, and the time has, I think, now come when, taking advantage of the experiments of various other bacteriologists, in addition, we may form more definite ideas as to its mode of

propagation, to its prevalence at particular seasons, and the causation of the symptoms present in its various stages, as well as to suggest some rational means for treating it.

I shall first refer to the typhoid bacillus and its discovery. In 1880 Koch and Eberth almost simultaneously discovered in the intestines, the mesenteric glands and lymphatics, and especially in the spleen of patients dying of enteric fever, a bacillus, which, though frequently found in the intestines as the common *Bacterium termo*, assumed at particular seasons, or owing to some alteration in the normal vital resistance of the individual, an acutely infective process, infiltrating the adenoid tissue and lymphatics of the intestine, accompanied by the chain of febrile phenomena we designate typhoid or enteric fever. Owing to the difficulty of finding a suitable nutrient medium on which to grow it outside of the body, this bacillus could not be differentiated from other bacteria inhabiting the alimentary canal until Koch succeeded in isolating and growing it on dry gelatine plates; since then all pathologists are familiar with its appearance and mode of growth. So far all experiments have failed in producing the disease from these cultivations by inoculation; but bearing in mind it grows both as an aerophyte and as an anaerophyte, it may be innocuous in the former condition and infectious in the latter when in the alimentary canal, when the vital resistance of the tissues in that situation may be from some reason altered. What may produce these alterations I shall allude to presently. Gaffky, in his observations on this bacillus, has given us some valuable information as to its situation and growth. It is one of the few bacilli found to develop freely in water, and it grows abundantly in milk. He also found it in the soil through which water percolates, and it grew freely in all albuminous media. He also found it more abundant in all these

media in the autumnal season more than any other—a fact of considerable importance. If, however, this bacillus is found so frequently in the food we eat, the water we drink, and in our intestines, how, we may ask, is it that it does not infect the intestinal glands when present, and produce enteric fever in every case? For we must all take it in at some time or other if it is not already present in our intestines. With this question I may link two others: why is it we do not suffer from pneumonia constantly when we always carry about with us the germs of the disease in Fränkel's diplococci? or why not frequently suffer from circumscribed or diffuse suppurations when the micrococci that produce them are frequently present in our blood or tissues?

Why we do not, recent investigations in bacteriology have made clear. Bacilli or micrococci are in themselves harmless either in the blood or tissues until the vital resistance of some tissue is lowered, from either functional alteration or injury, when they readily find a soil in it on which to grow and multiply. It is by this growth and the chemical products generated during it that the mischief is produced, and the poisoning of the tissues around that are most susceptible to its action afford a further field for the growth of these micro-organisms.

Let me take, first, the experiments of a distinguished physiologist, and, secondly, the course of a fatal disease, with which, clinically, we are only lately becoming familiar, to illustrate my meaning. Professor Kocher, of Berne, in making experiments on animals by destructive injury to tissues, down even to the marrow of bones, with a hot iron, could not produce septic inflammation as long as the animal experimented on was healthy, but if he lowered the vitality of it by feeding it on putrid matters, permitting thereby septic micrococci to enter its blood, a septic

inflammation was at once produced. The other disease I have alluded to in illustration—namely, septic or suppurative endocarditis—we know arises from either the staphylococcus or streptococcus when present in the blood from any accidental cause, finding a nidus in an inflamed endocardium or damaged valve, and the chemical product of its growth is then wafted in the blood current to set up mischief in other situations, where, from anatomical causes or lowered vital resistance, the blood and tissues cannot overcome its invasion. The germs of typhoid fever, like all other septic germs, are now regarded as in themselves harmless as long as the tissues with which they are in contact are healthy, else how can we explain the immunity from the disease that exists in healthy individuals who constantly either receive them through food or drink into the alimentary canal, or have them as normal and constant residents?

This brings me to the vexed question of the ætiology of the disease. If we take it for granted that the bacillus of Eberth is, by its infiltration of the glandular tissue of the intestine, the cause of all the mischief (and, apart from the absence of the inoculation evidence, most pathologists are agreed that this is so), what are the conditions that favour or produce its acute infection of those glands?

We have, first, the evidence that this bacillus is found growing most luxuriantly, and, we must presume, consequently that it is more virulent and more capable of making a vigorous battle for its existence, in the autumn. But it must find the tissues with which it comes in contact in a weakened condition to get the upper hand in the struggle.

Are the intestines at this period of the year in a more weakly condition than at any other, and, if so, from what cause? I think we can answer that question in the affirmative.

Most of us are familiar with the gastro-intestinal troubles that are characteristic of the early autumn months—the gastro-intestinal catarrh especially; the catarrhal or autumnal diarrhoea; and the frequency of so-called bilious attacks at this season. How these catarrhs are produced we have a ready explanation in the rapid fall of temperature in the evenings, after, perhaps, a warm mid-day, when the action of the skin is suddenly checked, and no additional precautions as to clothing are adopted. All hospital physicians are familiar with the sudden onset of bronchial catarrhs at this season also, from the same cause, and the rapid filling of hospital wards with cases of asthma, emphysema, and fresh broncho-pneumonia in cases the subjects of phthisis during the previous summer and spring. Now, let us see if this gastro-enteric catarrh is a usual precursor of enteric fever. Murchison, with that acute power of observation which was characteristic of him, mentions it as a most usual symptom preceding and accompanying the fever in its early stages; and he further adds that catarrhal diarrhoea was frequently present preceding the fever, and that it was often difficult to say whether the disease would remain catarrhal diarrhoea or end in enteric fever. Now, it seems to me that this catarrh, in addition to being brought about by atmospheric changes or by food, may be produced also from the absorption of the chemical products of the typhoid bacilli growing on the intestinal contents, when present in large numbers in either food or drink; and that this ptomain or toxin was the only weapon they used—as Professor Burdon Saunderson expresses it—in their struggle for existence to weaken the vital resistance of the tissues with which they were in contact, and make it fall an easy prey. Why, however, should the glandular tissue be the first overcome in this struggle? The reason seems to me obvious, looking at it from a

pathological point of view. Adenoid tissue is endowed with a very poor vitality, and very little power of resistance or of repair, when infiltrated or choked from any cause. We see this when it is attacked by a similar bacillus—the tubercular—whose life-history, and the readiness with which it infiltrates and destroys glandular structures, are somewhat similar to the typhoid one in many ways. The epithelial shedding and proliferation of the mucous membrane which take place must also weaken the defences against the bacillary invasion. The bacilli find the glandular tissue in a condition of derangement from the effects of this catarrh, and it becomes the centre of their habitation.

That this invasion is sudden, and followed by rapid changes in the glands there can be no doubt. Murchison, for example, found infiltration and swelling of the glands in the case of a patient dying on the second day of the disease, and other observers bear similar testimony.

The entire process in connection with the gland, so far as the typhoid bacillus is concerned, from the time of invasion to death or disablement of the gland, is over in fourteen days—the normal time of enteric fever from a pathological point of view. After this period, however, a new set of enemies appear on the scene in the shape of the suppurative micrococci, which, forming colonies on the injured or necrotic tissues around the glands, begin to generate their peculiar toxins, producing the hectic character of the symptoms and temperature with which we are all familiar after the first fortnight of the disease, so that we must regard enteric fever as the result of the growth in the intestines of two sets of micrococci—the so-called typhoid bacilli producing the symptoms during the first fortnight, and the suppurative micrococci producing the characteristic symptoms of its further stages. That the other

micrococci, as well as these latter, occasionally infect the patient from the intestinal canal there can be no doubt; and I have at present under my care in the Mater Misericordiae Hospital a patient with a well-marked attack of erysipelas of the face, in the fourth week of his enteric fever.

If recent bacteriological work has enabled us to grasp these facts with regard to the disease, we may ask ourselves what practical deductions can we draw from them, so as to enable us to treat it? Looking at the disease as primarily a catarrhal one of the intestines, and secondarily as one of septic poisoning, our treatment resolves itself into suitable diet and antiseptics. We know already how all-important is the treatment of enteric fever by bland and unirritating diet, and such as will be mainly absorbed by the stomach and duodenum, leaving little to be dealt with by the lower part of the small intestine.

The medicinal treatment of typhoid by antiseptics is latterly receiving that amount of attention which our more perfect knowledge of its bacteriological origin would suggest, and we see occasionally in the medical journals glowing accounts of the success of this method of treatment in the hands of some; while others confess it has not realised their expectations. As far as I myself am concerned, I have used this method of treatment for several years in both hospital and private practice, and I have every reason to be satisfied with the results.

I do not profess to believe that it will abort a case of typhoid once the characteristic fever has begun, but I do assert that it will prevent, in the majority of cases, the septicaemia—for it is nothing but septicaemia with which we have to deal after the second week of the fever is passed. The typhoid bacillus has by this time done its work, so far as the intestinal glands are concerned, and hereafter we have only saprophytic bacteria and their

effects to deal with. The characteristic hectic type of temperature during the third and subsequent weeks of enteric fever, such as we have in connection with suppurating cavities in the lungs or elsewhere, shows this to be the case.

In seeking for a suitable antiseptic for this purpose, we must choose one which will fulfil the following objects:— It must first exercise its effects in the intestinal canal, and not in the stomach; its action must be thorough, not alone antisepticiising the contents of the bowel, but it must permeate the intestinal wall as well, where septic micrococci may have already established themselves, and even enter the blood. To fulfil these conditions the form of antiseptic must be, in my opinion, a gaseous one. We know how readily the intestines absorb gases, and pass them into the blood. The antiseptic I am in the habit of using is chlorine in an alkaline solution, as in an alkaline form it mingles best with the contents of the intestines, which in enteric fever exhibit a strongly alkaline reaction. This treatment is not original, as Murchison, who expresses himself in general as dissatisfied with antiseptics, speaks favourably of chlorine, and regarded it as an admirable method of treatment. He administered it in an acid solution, which in my experience is not so satisfactory. That this method of treatment produces a fall in the temperature, and makes the type of the disease milder, there can be no doubt, and in over a fourth of the cases when begun early it brings the febrile process to an end about the fourteenth or sixteenth day.

Murchison, in his careful statistics, found only seven cases out of two hundred to terminate on the fourteenth day by the ordinary method of treatment, so the cessation of the fever by this method of treatment in such a large percentage must be more than a mere coincidence.

I do not claim for it that it is the best form of intestinal antiseptic, as more extended experience may enable me to procure a more beneficial one, and I hope on some future occasion to bring before the Section the results of my experiments on this subject in conjunction with our bacteriologist at the Mater Misericordiæ Hospital.

I have not, in these notes, entered the broader field of ætiological theories by trying to explain how enteric fever arises through the medium of sewers, or sewer gas; or why it is more prevalent among the wealthier classes than the poor, or whether it can arise as a miasma producing infection through the air—these are matters that I did not intend should enter into the scope of my paper, as I have been content to regard the disease from the bacillary point of view alone, and to see in it sufficient explanation of all the group of phenomena which we call typhoid or enteric fever.

[For the discussion on this paper see page 78.]

THE DIETETIC TREATMENT OF ENTERIC FEVER.

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[Read in the Section of Medicine, January 5, 1892.]

THE dietetic treatment of enteric fever is a subject well worthy of consideration, both on account of the frequent occurrence of the disease, and because there does not seem to be a perfect unanimity amongst the profession on the question, more especially in regard to the quantity of food which may be given.

Of the specific fevers which are commonly met with, enteric fever is remarkable for its tendency to relapse; the question, therefore, of the cause of relapses is an all-important one. On the view which each physician holds regarding the influence of errors in diet in bringing about a relapse will depend in great measure the care he takes as regards both the nature and quantity of food. If enteric fever is due, as is accepted, to a specific poison in the first instance, a true relapse, which is understood to mean a repetition of the illness—there being involvement of fresh Peyer's patches, a fresh outbreak of rose-spots, and a renewal of the fever—must be due to the same specific poison. This being so, a relapse must be brought about in one of three ways:—1. Exposure to fresh infection; this view has been advocated by some writers. 2. Part of the original poison remains somewhere in the system, and has not been developed. 3. The poison thrown out into the intestinal canal in the course of the disease, or that which has been absorbed into and lodged in the mesenteric glands, reinfects the patient. The

first theory—viz., that all relapses depend upon exposure to a second infection, either by the patient having remained in the same place where he originally acquired the infection and so having become infected again, or by two distinct infections having occurred during the period of incubation before the outbreak of the primary illness (the primary illness having been short, and so the susceptibility to the disease not having been exhausted) is shown by Liebermeister to be untenable; for, he argues, patients get relapses who are removed from all sources of fresh infection, and relapses occur in many cases after a primary attack of ordinary duration; consequently, this view, even if it may explain some cases, cannot account for all. Liebermeister's conclusion is—"Relapses are usually due to the same infection that caused the original attack; part of the poison must have remained latent somewhere in the body, not developed during the first attack, not destroyed or expelled, and not beginning to mature until afterwards."^a This is the second possible cause of relapse. The third possible mode by which a relapse is brought about—viz., reinfection by the products thrown out in the course of the illness—has been advocated by MacLagan, who believes that healthy glands become inoculated by the slough thrown off by those first affected, and he maintains that relapses are met with only when there has been constipation during convalescence. In opposition to this view, the observations of others tend to show that relapses occur just as frequently when there has not been constipation, and a further difficulty in accepting this theory is pointed out by Murchison—viz., that the glands affected in relapses are higher up than the sloughing glands involved in the primary illness, and consequently, inoculation by the slough can hardly occur. Possibly the mode by which a relapse is brought about may be different in different instances. Some patients

^a Von Ziemssen's *Cyclopædia*.

may get a second infection ; in other cases, a part of the original poison may remain undeveloped until after the primary illness is over ; while in others the poison thrown out in the course of the disease may reinfect—but, whatever be the true explanation, one point is clear, a relapse implies that the susceptibility to the disease is not exhausted in the patient who is the subject of the relapse. The question at once arises—Why are relapses so much more common in enteric fever than in the other specific fevers—*e.g.*, typhus, measles, scarlatina ? I have only once seen a relapse in measles. This occurred in a man who was admitted into the Adelaide Hospital a few years ago, suffering from measles ; he had been only ten days convalescent from the same disease, for which he had been under treatment in Cork-street Fever Hospital. Bristowe, in his “Practice of Medicine,” alludes to such cases as occasionally, though rarely, occurring.

Before proceeding to the discussion of the question—“Why are relapses so common in enteric fever as compared with other fevers?”—another question presents itself, *viz.*, “How can one be quite sure in a given case that a true relapse has taken place, and that the symptoms are not due to further changes taking place in the already diseased intestinal glands and not to fresh changes occurring in glands which escaped in the primary illness ?” I think it is by no means an easy question to decide, and, in many cases, I believe, so-called relapses are merely recrudescences of the primary illness. We find, on examining the statistics of different writers on enteric fever, a strange variation in the percentage of relapses. Thus, Biermer had relapses only in $3\frac{3}{10}$ per cent. of his 1,136 cases. Bäumler in 73 cases had relapses to 11 per cent. If we are to consider that an apparent convalescence—as evidenced by apyrexia for a few days, disappearance of spots, and a general improvement in the condition of the patient, followed by fresh fever of fourteen to twenty days

duration and fresh spots, is a certain proof that a relapse has occurred, no doubt it occurs commonly; but if we study each of these points separately we shall see that the diagnosis of a relapse is not easy. For, first, as regards the temperature, the fever sometimes comes down early before the illness can have had time to run its course; in such a case a second rise could hardly be looked upon as a relapse. Secondly, if the spots are few, it is possible that the old ones may disappear before a fresh crop appears; and if the interval between the two crops coincided with a normal temperature, the case might look like a true relapse. Moreover, there is often a return of appetite and an apparent convalescence early, in cases when we can hardly feel justified in considering that the primary illness is over. Murchison alludes to a statement of Sir William Jenner (to which he takes exception), who expressed the opinion that, except in a case of relapse, fresh spots never appear after the thirtieth day, and that febrile symptoms after that date are always due to some incidental complication. Should the fact of apyrexia occurring after a period of four weeks be an invariable proof that the primary illness is over, the diagnosis of relapse would be easy; but we cannot lay down any definite duration for the primary illness. Many cases last much longer than four weeks without any apyrexial period. Therefore, we are forced to regard this prolonged pyrexia as due to the primary illness, unless we are to adopt the suggestion thrown out by Murchison, as a possible explanation of prolonged cases—viz., that in these cases a true relapse may overlap the primary attack without any apyretic interval.

Lastly, the proof that there is such a condition as a true relapse depends upon the examination of the intestines in fatal cases. Even then, care must be exercised in the examination, as it is, I think, common to find, in cases which are fatal during the primary illness, glands ulcerated in the

lower part of the small intestines, and swollen, without ulceration, higher up—the involvement of the glands proceeding in the order, as to time, from below upwards. The only pathological proof, therefore, that convalescence had taken place, and was followed by a relapse, would be the presence of cicatrising ulcers along with swollen glands, and the fact that so long a time had elapsed from the commencement of the illness that glands, primarily affected, could not be in a state of simple swelling only. Trousseau, as Murchison states, denied that any fresh disease of the bowel occurred. Trousseau writes—“To explain the intensified returns of the fever, and the successive eruptions, we must suppose that the morbid poison has not exhausted itself in the first outbreak, and that the economy, to get rid of it, requires repeated efforts. These returns of the fever are neither relapses, nor still less are they new attacks; it is the same attack, the symptoms of which, temporarily interrupted, recur under the influence of the same morbid cause which produced them in the first instance. However complete the symptoms may be, and although the eruption reappears, the characteristic intestinal lesion never returns.” This view is not accepted by Murchison and other competent observers. If we grant, therefore, that relapses do occur and are common, the question, as stated above, arises—“Why are they so much more common in enteric fever than in the other specific fevers?”

The fact that the lesions in enteric fever are in the intestines, and so are exposed to the direct contact of food more or less altered by digestion, has naturally suggested the theory that relapses are brought about by errors in diet. Most writers on enteric fever seem to give little countenance to this view, as they fail to comprehend how an illness due to a specific poison can be reproduced by any other influence except the poison. Certainly, it is hard to understand how

errors in diet can act in bringing about a relapse. I feel convinced, however, that some, at least, of the relapses I have seen were due to errors in diet. In two of my cases solid food, brought surreptitiously by friends of the patients, seemed to act as the cause. Liebermeister, who believes that errors in diet may bring about a relapse, remarks—“As a matter of course, the injury produced by any such means can only serve as an exciting cause to processes that follow, stimulating the development of germs which would otherwise have remained dormant for a time, or which might even have been eliminated from the body.”

An argument that if true relapses can be brought about by errors in diet they should occur oftener, may be met by the answer that errors in diet, to produce a relapse, must be made in cases of patients whose susceptibility to the disease is not exhausted.

Whatever view may be held as to the genuineness of so-called relapses, and whatever theory may be held as to the cause of relapses, I think the necessity for care in diet, considering the position of the lesions, must be admitted. It is impossible not to believe that grave injury may be inflicted upon the inflamed and ulcerated bowel by want of care in both the nature and quantity of food. Still, I believe, sufficient care is not exercised in this matter. While most physicians are agreed upon the advisableness of liquid food, many show too little regard as to the quantity of this liquid food which they allow. Indeed, it seems to be the idea of many practitioners, to judge of them by their practice, that provided they are careful as to the nature of the diet, the quantity is of no great importance; the patients are allowed to take as much as they or their attendants desire. Most text-books say little about the quantity permissible; yet the quantity of food permissible is just as important a consideration as its nature, and much harm may be done by excess of

allowable food. It is pleasant to see that Dr. J. W. Moore, in his recently published "Text-book of the Eruptive and Continued Fevers," does not lose sight of this important point. I shall consider first the kind of food, and then the quantity of food permissible.

Milk is, unquestionably, the best and safest food for enteric patients; given diluted with a little soda-water, lime-water, or plain water, it agrees well with most patients. If there is diarrhœa, it is best to give it boiled; otherwise boiling is unnecessary. Sometimes it is necessary to peptonise the milk. If a patient can take milk, milk alone is the best food during the entire illness. If milk is vomited, or curds are passed by stool, whey may be advantageously given; if whey is given, beef-tea with the grounds, or beef-juice, should be given along with the whey to take the place of the albuminate casein. With regard to beef-tea, some object to its administration on the ground that it causes gaseous decomposition in the intestine, and so increases the tympanites. I have not found this to occur. I consider that beef-tea is a useful addition to milk diet when the patient is unable to take a sufficient quantity of milk. It seems also to act as a stimulant.

Farinaceous foods are often given. I am not in the habit of giving them to enteric patients. I object to them on two grounds—first, I think they may cause troublesome flatulence, and may increase the fever, and, if given early in convalescence, may cause a fresh outburst of fever; secondly, they are not needed when sufficient milk can be taken. In a severe attack of enteric fever the salivary glands are quite inactive, and certainly no digestion of starchy food can take place in the mouth. No doubt, in health the saliva plays an insignificant part in the digestion of starch food; still, it is, perhaps, not unlikely that when the salivary glands are inactive, the amylolytic ferment of the pancreas is inactive

too. If starchy food is given, it can be given only in a thin semifluid form, except in the mildest cases.

Beaten-up eggs are given by many physicians. I seldom give them. I think when sufficient milk can be taken, eggs are unnecessary, and I am doubtful if they are well digested by enteric patients. Of course, when patients cannot take milk the administration of eggs may be absolutely necessary. They ought, then, to be given raw, beaten-up, and well diluted.

Having considered the nature of the food permissible, I shall now consider the quantity. In my opinion, two to three pints of liquid nourishment in the twenty-four hours is ample food for adults, less, proportionately, for children. By care in the quantity and the nature of the food, many of the symptoms which are liable to give trouble are more easily controlled.

Diarrhœa.—Avoidance of excess of liquid food is the best general means of counteracting this symptom. In my experience I have had very little trouble from diarrhœa, and I am convinced that this has been due mainly to the care taken in avoiding excess of food. Patients have been under my care in the Adelaide Hospital who before their admission were allowed what I should consider a great excess of food, and who had excessive diarrhœa; these patients on restricted diet were almost at once relieved of this symptom. One patient, who before his admission to the hospital had been getting two quarts of milk and three beaten-up eggs, with stimulants in addition, in the twenty-four hours, and had profuse diarrhœa, was no longer troubled with it on restricted diet. I have had constipation to deal with much more frequently than diarrhœa. With regard to constipation, I think it is well not to allow more than two days to pass without a motion from the bowels, unless the patient has had a severe hæmorrhage previously. There is one important reason for

ensuring an action from the bowels at least every second day—viz., to avoid trouble from accumulated fæces during convalescence. In cases where there has been constipation I have known distress occur during convalescence from hardened fæces in the colon. The objection that when the diet is limited as I have indicated constipation results, is, I consider, of little weight, as constipation can very easily be remedied.

Hæmorrhage.—This symptom, like diarrhœa, is best guarded against by restricted diet. A man under my care in the Adelaide Hospital got a severe hæmorrhage from his bowels, which followed directly the administration of an excessive quantity of milk, the nurse having yielded to his clamour for abundance of milk. For several days after the hæmorrhage I feared to allow him more than one pint of whey, with stimulants, in the twenty-four hours. He made a good recovery. Excess of diet will predispose to hæmorrhage by increasing the inflammatory changes in the ulcerated bowel.

Tympanites.—This symptom is kept under control by limiting the quantity of food. By care in the diet tympanites does not often give trouble.

Sleeplessness.—It is seldom necessary to resort to large doses of hypnotics when care is taken to avoid excess of food. Patients sleep much better on a carefully restricted diet, and often no hypnotic is needed.

Delirium.—I think if excess of food be permitted, delirium will give more trouble.

High Temperature.—Perhaps unnecessary alarm is sometimes shown if the temperature touches a high point, 104°–105° Fahr., and perhaps harm may be done by too hasty a resort to antipyretics. Antipyretics are seldom called for (with the exception of the always useful and grateful tepid sponging), unless the temperature remains persistently high morning and evening for a few days—a high evening tem-

perature, 104°–105° Fahr., while the morning temperature is one or two degrees lower, does no harm. Of course if a very high temperature is reached, antipyretic remedies must be used. I think the temperature is also, like the other symptoms above discussed, influenced by the quantity of food. Dr. Head has told me that he often observed a marked improvement to occur in the general symptoms when from any cause, such as threatened hæmorrhage, the quantity of food had to be reduced to a minimum. The question arises—“Is the limited quantity of food above recommended sufficient for the patient’s nourishment?” Well, it is very doubtful if, when the digestive powers are so much impaired as they are in enteric fever, a patient can digest more; consequently, any food which escapes digestion must act as an irritant to the diseased intestine. I cannot do better than quote Broadbent in this connection:—“The attention of the medical attendant will in most cases be required rather to moderate the amount of food given than to urge its administration. . . . The key to the regulation of the diet—it may almost be said to the management of the patient—is to be found in the careful inspection of the stools; . . . when curds appear in the dejections, either too much milk is taken, or too much at a time, or its digestion is interfered with. If the passage of undigested milk is not remedied, there will certainly be flatulence, discomfort and restlessness, elevation of the temperature, and in most cases diarrhoea.”^a Broadbent recommends as the diet, two to three pints of milk, and a pint or pint and a half of beef-tea or some equivalent; this is rather more than I am in the habit of giving—his maximum being four and a-half pints of liquid food—but his diet is very restricted as compared with what is allowed by many practitioners.

Another matter for consideration is, “May simple drinks

^a Typhoid Fever—Quain’s Dictionary of Medicine.

be allowed *ad libitum* to relieve thirst?" Sir William Jenner^a says:—"Patients suffering from typhoid fever should be allowed an unlimited supply of pure water." Broadbent allows this also; he writes:—"The patients should, of course, be allowed to drink freely of cold water, toast-water, or any simple drink." With all deference to so great authorities, I am disinclined to sanction such great freedom, and, indeed, I cannot say that I have found thirst so very distressing as to call for an *unlimited* supply of simple drinks. I should fear that unlimited freedom in the use of simple drinks would cause abdominal discomfort, and, perhaps, diarrhoea. Water in moderate quantity, pieces of ice, and a little lemonade are, I think, sufficient. Tea, morning and evening, is also very grateful and safe.

Whatever diet is adopted in a given case, it is, I think, well not to alter it much in the course of the illness, as the digestive tract in enteric fever is very sensitive to alterations in food. The plan of treating enteric patients, as one would treat a chronic invalid—viz., making alterations in the food from day to day—is, I think, dangerous, and certainly not called for.

There is another point in treatment which is worthy of attention; it is this—towards the end of the third week, when the ulcers are formed, it is often well to lessen somewhat the quantity of food.

A very important consideration is, "When may a patient be regarded as convalescent, and allowed a change in diet?" The guides are, the presence or absence of spots, the temperature, the tongue, the condition of the abdomen, the size of the spleen, and the stools.

1. *The presence or absence of spots.*—I am sure that all physicians are agreed that though the patient may appear convalescent, and every febrile symptom be gone, the

^a Treatment of Typhoid Fever.—Lancet, Nov. 15, 1879.

presence of enteric spots is proof that the illness is not over. One occasionally sees enteric spots, when every other symptom of the illness is over. Of course, no conclusion can be drawn when spots are absent, as they are often absent all through the course of enteric fever.

2. *The temperature.*—This is a very important guide. Murchison writes:—"Convalescence can only be said to be fairly established when the temperature is normal on two successive evenings." The temperature should, I think, be normal for at least a week both evening and morning before a change in diet be allowed. It is well to take into consideration also the mode in which the temperature has come down before it reaches the normal; whether very gradually or quickly; if the temperature has fallen gradually, a week of complete apyrexia is generally sufficiently long to withhold more food; if there has been a rather sudden fall, this time is not sufficient. But though the temperature is a very useful guide, it is not an infallible one. Cases of enteric fever occur in which the temperature remains normal throughout the greater part of the illness; and, on the other hand, complications may arise during the illness, the effects of which may last after the enteric lesions have healed; which complications, while they keep up the fever, do not necessitate a restricted diet. To sum up: a normal temperature is an important, but not a certain guide that convalescence has begun; a high temperature, provided there is no complication present, is an almost^a certain guide that convalescence has not begun.

3. *The condition of the tongue.*—I think that the only help that examination of the tongue affords as to whether convalescence is established or not is this: if the tongue

^a I say "almost," as occasional exceptions do occur, in which the temperature remains above the normal, unaccountably, long after every other symptom of the illness is over.

is dry, the illness is not over; it generally becomes clean when convalescence commences, but certainly not always so; the coating will, in many cases, not clear off until solid food is given, and then it is often remarkable how rapidly the tongue cleans.

4. *The condition of the abdomen.*—It must have lost its tympany.

5. *The size of the spleen.*—The examination of the spleen is most important. Excepting cases in which the spleen has been, previously to the illness, enlarged from some chronic condition, its enlargement contra-indicates a change of food. I have more than once seen patients, apparently convalescent, but with a palpably enlarged spleen, get a return of fever, when I was tempted by the disappearance of the other symptoms to allow a change in diet. With regard to enlargement of the spleen in enteric fever, Liebermeister writes: "Gerhardt makes a statement that in many cases in which a relapse takes place, the enlargement of the spleen is not at all diminished during the non-febrile period that intervenes between the original attack and the relapse." Perhaps in many of such cases of so-called relapse, the condition is rather a continuance of the primary illness, with a fresh outburst of fever. I should be disinclined to consider convalescence as begun if the spleen remained palpably enlarged.

6. *The character of the stools.*—If they are fluid and light in colour, convalescence has not begun.

The frequency of the pulse does not serve as a guide, as it may not only be slow during the entire illness, but it may be frequent during convalescence. Murchison writes: "The pulse may be quicker than during the fever." I have noticed this increased frequency in some cases. To recapitulate: Convalescence from enteric fever may be regarded as begun—if there are no spots present, if the

temperature is normal for at least a week, if the tongue is moist, if the abdomen is not tympanitic, if the spleen is not enlarged, and if the stools are formed and of a natural colour.

When convalescence has begun, the next consideration is as to what is the safest way to change the diet. I think that the safest way is to increase very gradually the consistence of the food. A sudden change from liquid to solid is often followed by a fresh outburst of fever. The digestive tract, long accustomed to liquid food, cannot tolerate, as a rule, a sudden great change in its consistence. For a few days, indeed, it is safer to increase the quantity of the food which has been given during the illness rather than to make any change in its nature. I change the diet much as follows:—first, I give milk thickened with corn-flour or arrowroot; then, after a few days, a lightly-boiled egg and a few plain biscuits, then bread, then fish, and so on; all along I watch the temperature carefully, so as to detect at once any rise of temperature, should it occur, as a rise would indicate, in most cases, that the food was being increased too rapidly, or injudiciously.

A very natural question may be asked: “Granted that many cases require such extreme care in diet, is it necessary to be so particular in every instance? May not more laxity be allowed in mild cases?” This question is best answered by another: “What is meant by a mild case?” If it is meant a case with mild symptoms, laxity may not be allowed without risk, as, undoubtedly, many apparently mild cases occur which suddenly get grave symptoms and die, and *post-mortem* examination reveals an amount of disease out of all proportion to the earlier symptoms. Such surprises may not be common, but they do occur. If it is meant by a mild case a case with only slight intestinal disease, more laxity may be allowed; but how are such

mild cases to be recognised with perfect certainty? They cannot be. Therefore, if it is granted that care in diet is necessary in some cases, it is best to take it in all. I believe that while it may be true that those who give abundance of food in every case, or those who allow such abundance in the apparently mild cases, are likely to have more rapid recoveries than those who do not, the latter are more likely to have fewer fatal cases.

The experience I have had has led me to believe—

1. That most cases, if not all, are best treated by liquid food in limited quantity—at most three pints; this food being chiefly or entirely milk.

2. That by thus limiting the quantity of liquid food, diarrhoea, hæmorrhage, tympanites, sleeplessness, and delirium will be more easily controlled.

3. That errors in diet cause frequently a renewal of the illness, which in its clinical features is exactly what most writers on enteric fever would regard as a relapse.

I have dwelt so strongly on the dietetic treatment of enteric fever, because I consider it a matter of very great importance, and because it is mainly as regards diet that physicians differ from each other in their treatment of this disease. I do not mean to imply that I consider other points in the treatment of enteric fever as of little importance.

[For the discussion on this paper see page 78.]

A CASE OF RECURRENT ENTERIC FEVER, FOLLOWED BY TRUE RELAPSE.

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[Read before the Medical Section, February 5, 1892.]

THE present "Clinical Record" may well claim the attention of physicians, inasmuch as it refers to a case in which, not only did enteric fever recur in the same patient, but the recurrent attack was succeeded by a true relapse after an apyrexial period of eleven or twelve days.

In April, 1877, I attended with the late Dr. Alfred Hudson and Sir George Owens, a lad of fifteen years, who passed through a typical attack of enteric fever lasting 23 days—in which, however, constipation and not diarrhoea was the rule. In October, 1891, the same gentleman, at the age of twenty-nine, sickened of a fever which proved to be undoubted enteric fever. After running an acute course of 24 days, followed by a subfebrile period extending over another week, this attack was succeeded by convalescence, which seemed to be in all respects normal. On the eleventh or twelfth day, however, from the establishment of apyrexia, acute febrile symptoms again showed themselves, and for the third time in his life the patient passed through an attack of enteric fever.

The salient points are of course only given in the foregoing brief statement. The facts are as follows:—

CASE.—In April, 1877, W. H. P., a schoolboy, aged fifteen, was staying in the Co. Wexford, where he slept in a room situated near an offensive manure-heap. To this the patient himself attributed

his illness, which began on Thursday, April 19, with a feeling of nausea at school. He returned home and went to bed. He at once lost all appetite, the sickness continued, but without vomiting. The following day diarrhœa set in after a dose of effervescing magnesia; this was in turn succeeded by constipation.

On Tuesday, April 24 (6th day), he was seen by Dr. Hudson, Sir George Owens, and myself. At 1 30 p.m.—pulse 94, resp. 34, temp. 103·3°. His tongue was furred, but moist; some sordes clung to the teeth. He was perspiring freely; there was no abdominal tenderness. Next day rose-spots appeared, and the fever ran very high, the thermometer marking 103·7° in the axilla at 6 p.m. At this time he was on an exclusively milk diet, and the bowels were obstinately confined.

On April 28th (10th day) I took this note at 6 p.m.:—"Has been more prostrate this afternoon—P. 86, Resp. 34, T. 102·7°. To have chickenbroth, claret, and this mixture:—

R. Extracti Cinchonæ Liquid. ʒii;
 Acid. Hydrochlorici diluti, ʒii;
 Tincturæ Cardamom. Comp., ʒss;
 Aquæ Chloroformi, ad ʒvi.

M. ft. mist. Signetur: Half an ounce three times a day."

On April 29 (11th day) the state of the patient was still less satisfactory—the tongue was drier and browner, more sordes had gathered on the teeth and gums; there was an icteroid tinge of the surface. He had not passed water since 8 p.m. of the previous day. The belly was fuller and more tympanitic; the area of splenic dulness was increased. Many rose-spots were seen on the chest, abdomen, and arms. The favourable points about the patient were that he had slept well, and pulse, respiration, and temperature were all falling uniformly. At 3 p.m. he was hot and uncomfortable, owing to inaction of the bowels and retention of urine. I gave a large warm water enema, when quantities of solid and fluid fæces came away, and he passed water freely twice. The tongue was much coated at this time, and the papillæ were enlarged and fiery.

On May 2 (14th day) the temperature rose to 103·4°, and on the following day, after 4½ hours' continuous sleep, the urine, which was dark-coloured and of high density (sp. gr. 1025), began to throw down copious deposits of urates. This occurrence was manifestly of the nature of a crisis, for the patient's state improved daily henceforward, although it was not until May 12th (24th day) that I was able to make the entry in my notes, "No fresh rose-

spots." A period of subnormal pulse and temperature readings now followed—the pulse falling to 54 beats per minute, and the temperature to $96\cdot5^{\circ}$ on May 20th (32nd day). Two days previously I had made this entry—"Slept well, *whistled* this morning," not a bad sign of convalescence in a school-boy.

Soon after this the patient went to Caunterets in the Pyrenees, where his health was happily quite restored.

October 15, 1891.—On this day Mr. W. H. P., aged twenty-nine, now a barrister by profession, left Dublin apparently in good health, on a week's visit to a country house in one of the midland counties of Ireland. He returned on Thursday, Oct. 22nd, and two days later, in the evening, he first complained of feeling unwell. From this statement I conclude that Mr. P. probably contracted the fever in his town residence before he went on a visit—the most usual length of the period of incubation in enteric fever being a fortnight or thereabouts.

On Sunday, October 25th, the patient awoke with a stiff neck and a slight headache. Next day he went out, although feeling badly, and in the afternoon sat over the fire owing to an unconquerable chilliness. On Tuesday, the 27th, he was utterly unable to leave his bed. He spent the next three days in bed, attended by Dr. L. H. Ormsby, who found his morning temperatures 100° , his evening temperatures 101° , or slightly higher. Feeling rather better on Friday, the 30th—when both morning and evening temperatures were $98\cdot5^{\circ}$ —he got up at 3 30 p.m., and sat in an arm-chair at the fire, feeling very weak. At 7 p.m. he ate partridge on buttered toast for dinner, after which he spent a restless night, so that by the morning the temperature had risen to 101° , remaining at this point throughout the day. I saw him with Dr. Ormsby in the evening, and came to the opinion that the attack had probably arisen from overfatigue in the country and indigestion. Having a vivid recollection of his enteric fever $14\frac{1}{2}$ years previously, I put aside the thought that it was the malady from which he was now suffering.

At 9 a.m. next morning (November 1) the temperature was 104° , the patient was coughing frequently, especially whenever he lay on his right side. He brought up mucous sputa, occasionally streaked (not tinged) with blood. Diarrhœa now set in—five motions of the colour and consistence of pea-soup occurred in the 24 hours ending at 9 a.m. of November 2, when the temperature was 103° . Shortly afterwards it rose to $103\cdot7^{\circ}$, with a pulse-rate of 104, and respira-

tions 30 to 32. He was ordered to be sponged in the morning, afternoon, and evening, and at midnight, while milk and lime-water, and chicken jelly, followed by crushed ice, were given alternately.

November 3 (11th day).—Rose-spots are visible to-day, both on the abdomen and on the back. The patient's dietary now consisted of 3 ozs. of boiled milk with 1 oz. of lime-water every third hour, and 2 ozs. of chicken jelly every sixth hour. Two turpentine capsules (containing 10 minims) were given every fourth, and a three-grain quinine pill every eighth hour. Crushed ice was allowed freely, and the tepid sponging was carried out as before.

November 4 (12th day).—Increasing bronchial trouble was treated by the free application of compound camphor liniment to the back and front of the chest, which was then enveloped in cotton wool, previously well warmed. The hip, back, knees, and other parts exposed to pressure were rubbed with spirit of camphor night and morning, and then dusted with "violet powder."

November 5 (13th day).—Temp. 103°. The only change in the treatment was to add one ounce of beef tea to each meal of chicken jelly.

November 6 (14th day).—Evidences of serious heart-failure now presented themselves. The cardiac sounds were weaker. The heart changed its position easily—when the patient turned from side to side, the heart "sagged" over to the dependent side. The area of pre-cordial dulness had increased laterally, and the radial pulse failed when the arm was held aloft. The patient spent a very restless night. Morning temperature, 103.2°. One ounce of brandy was given in teaspoonful doses during the day.

November 7.—A complaint of pain in the abdomen was met by the application of turpentine fomentations.

November 9.—We found the patient weak after a restless night, with some delirium. Ordered to have food every hour—either one ounce of hot milk with a teaspoonful of brandy, or 1½ ozs. of mixed chicken jelly and beef tea with 2 teaspoonfuls of Brand's essence, washed down by half an ounce of old port.

On November 13 (21st day), rennet was ordered for a change, as the bowels were rather constipated.

November 15.—The yolk of an egg was put into a cup of tea morning and evening.

During the following few days enemata were administered to overcome constipation. On November 20 rusk was broken into the beef tea. The dietary now was:—A cup of tea and yolk of an

CHARTS OF TEMPERATURE RANGES IN RECURRENT ENTERIC FEVER.

Fig. 1.—First Attack, April, 1877. Patient aged 15 years.

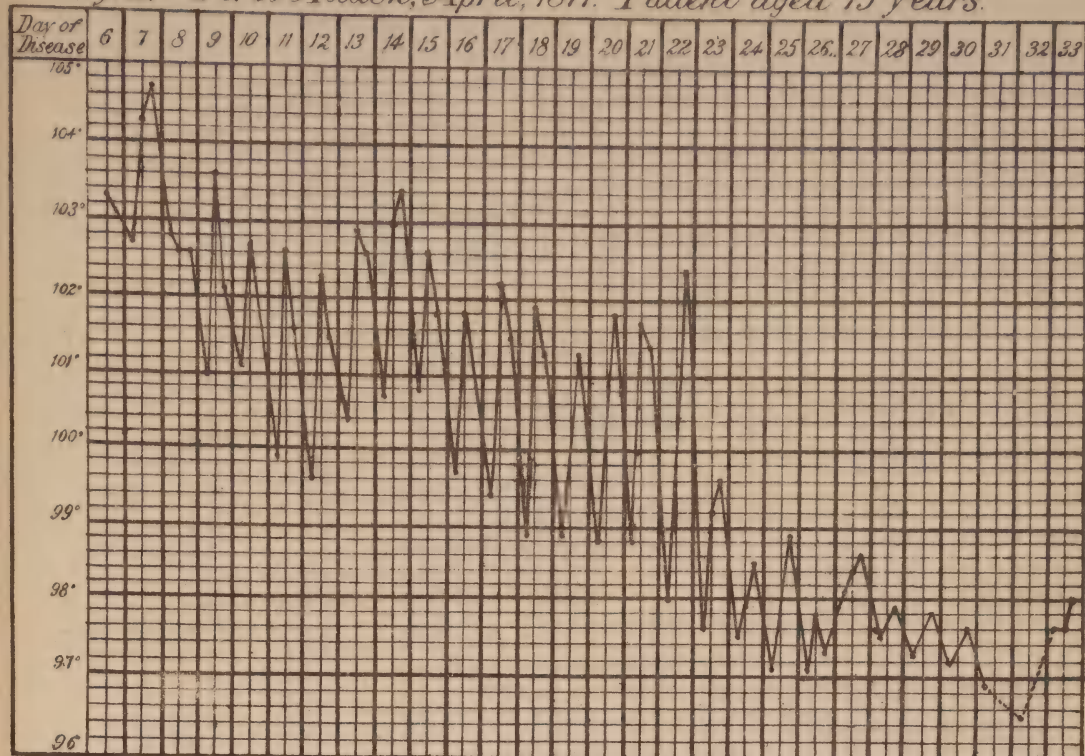


Fig. 2.—Second Attack, October, November, 1891, Patient aged 29 years.

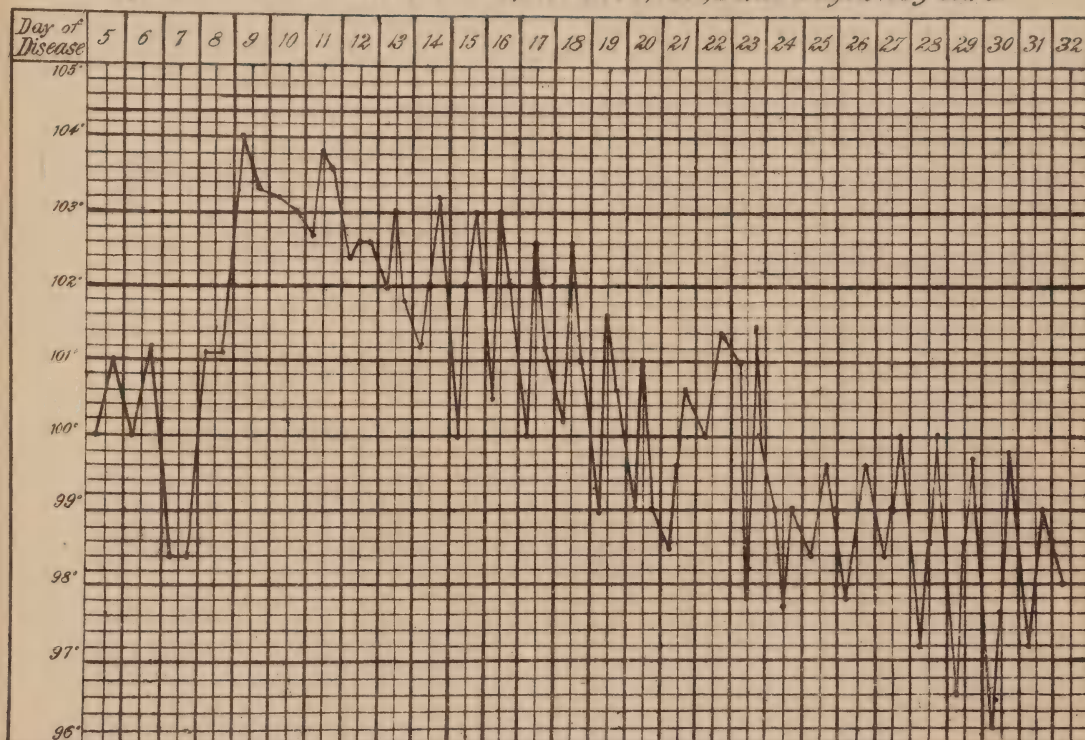
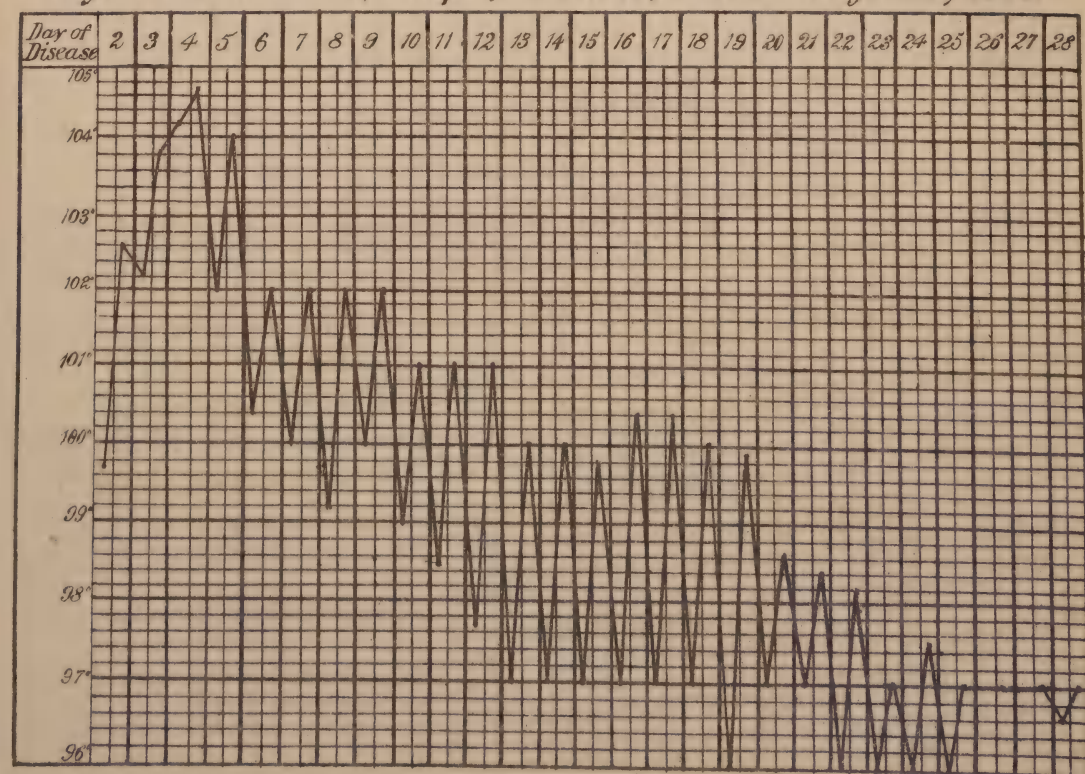


Fig. 3.—Third Attack (Relapse) December, 1891. Patient aged 29 years.



egg at 9 a.m., a cup of beef tea at 11 a.m., a cup of custard at 1 p.m., a cup of rennet at 3 p.m., a cup of tea and egg at 5 30, a cup of beef tea at 8 p.m., and so on during the night, if awake.

November 22.—Sat up for a few minutes while bed was re-made; seemed wonderfully strong.

November 24.—Got thin bread and butter without crust morning and evening, with cup of tea; sat up in armchair for one hour.

November 27.—Walked into adjoining room, and sat there for two hours; temperature subnormal this morning.

November 29.—Walked down to the drawingroom to-day, and sat up for two hours all medicine stopped, and plenty of nourishment given; porridge and cream at bed-time.

December 3.—Got minced chicken for first time to-day, and enjoyed it.

December 4.—Went out for half an hour in the carriage.

December 6.—Had filleted sole for dinner and baked apples, also a glass of wine.

December 8.—Patient's cough has disappeared; he eats and sleeps remarkably well; is gaining strength, but still looks thin, as most typhoid patients do in the stage of convalescence.

December 9, 1891.—The patient's temperature had been subnormal in the morning for three weeks. He had been downstairs, in apparently good health, and eating such solids as chicken, fish, and bread and butter. His only complaint was of constipation. Each morning about 10 o'clock he had one very small motion, always composed of lumps, passed either singly or sometimes in a hard, large mass. This was duly reported as well as marked on the daily chart.

In the afternoon of this day the patient saw his first visitor, a friend who stayed with him from 4 to 4 45 p.m. He seemed to enjoy his company very much, laughing and talking pleasantly. At 5 30 p.m. he had tea. At 7 p.m. he went to bed. At 8 p.m. he said to the nurse:—"I fear I have talked too much this afternoon, I have a headache again." The nurse took his temperature, and found it 100°. A dose of 30 drops of liquid extract of cascara sagrada was given, and acted well by morning, but at 9 a.m. the temperature was still as high as 99·8°. Notwithstanding, the patient dressed as usual, came down to drawingroom, and even went out for a drive in the middle of the day. He dined on filleted sole.

Towards evening he became very feverish, the thermometer rising to 102·8°; he was sponged, after which he slept well. Next

morning he was dull and heavy-looking, complained of headache and pains in his arms; the temperature was 102° . I saw him with Dr. Ormsby, and considered that the attack was one of influenza—basing my opinion on the sudden onset, the neuralgic pains, the thickly-coated tongue, and the rapid, running pulse. The temperature went on rising, however, the fever culminating on the evening of the fourth day in a reading of 104.6° . On the following day the reappearance of rose-spots in considerable number left no room for doubt as to the nature of the case, which was clearly one of relapse in enteric fever. At this time the temperature ran very high, the tongue was very dirty and coated, headache was severe, and there was much delirium at night, the patient imagining that other sick people were in the room with him, and asking the nurse if she had fed them also. There was albuminuria, and the urine was concentrated and loaded with urates.

One day the patient complained of sore throat, and we found that the tonsils were enlarged, while the uvula, soft palate, and pillars of the fauces were red and very œdematous. Much pappy exudation lay free on the surface of the mucous membrane. The throat symptoms yielded readily to frequent spraying with lactic acid, the “nebula” employed consisting of one drachm each of lactic acid and glycerine with 14 drachms of water.

The course of the relapse fever is shown in Fig. 3 in the Plate of the temperature charts. From December 12 to 16 the temperature was 102° each evening, falling to 100° in the mornings; afterwards for a week it was subnormal each morning, but rose to 100° in the evening.

December 30, 1891.—The patient is, happily, once more very well, eating solids, coming downstairs, and feeling wonderfully strong. Constipation set in with convalescence as before, but on this occasion a teaspoonful of castor oil and one of glycerine were given in milk every second morning with a good result—this simple dose keeping the patient comfortable and regular.

January 7, 1892.—The patient's temperature, which had been persistently subnormal for at least ten days, began to rise a little—from 96° to 98° in the evenings.

January 25, 1892.—On the evening of this day the patient, now quite convalescent, sailed for Holyhead *en route* to Torquay.

Remarks.—There can be no question that in a large majority of instances one attack of enteric fever confers a life-long

immunity from a second. Murchison quotes Bretonneau, Gendron, Chomel, Louis, Piedvache, Sir William Jenner, William Budd, and Bartlett as holding a belief in the doctrine of acquired immunity as applied to enteric fever. At the same time well-authenticated instances of persons contracting this fever a second time are on record. Several have come under Murchison's own notice, in which both attacks occurred subsequent to puberty. Trousseau records two examples of a second attack—one patient, a woman, suffered again after an interval of four years; the other, a girl, had a severe attack at the age of twelve, and another, equally severe, a year afterwards. Similar instances of unequivocal second attacks have been recorded by Piedvache, Michel, Bartlett, Paul, and William Budd. To this list, I submit, the case now reported may with propriety be added.

On November 5th and 6th, 1891, I had the advantage, through the kindness of Professor Emerson Reynolds, and with the aid of Mr. Emil Werner, of testing the diagnostic efficacy of Ehrlich's very striking diazo-test in a series of specimens of urine. Two of the series were non-typhoid urines, two others were from enteric fever patients, and the fifth was the urine of the gentleman whose case is the subject of the present clinical record, on the 13th day of well-marked enteric fever, he having suffered (as has been already stated) from equally well-marked enteric fever $14\frac{1}{2}$ years previously, when a lad of 15 years. In all cases a change of colour in the urine was observed on application of the test. In the non-typhoid urines only a deeper yellow was produced; in the undoubted primary typhoid urines a beautiful rose coloration quickly developed. In the case of recurrence, changes intermediate between these extremes were noticed—it was as if the previous attack exerted some controlling influence over the reaction.

Perhaps the most interesting feature in the case detailed

in this communication is the *relapse* which followed hard upon the second attack of enteric fever. In my long experience of enteric fever, I can recall only three cases of true relapse. One of these I reported to the Academy of Medicine in Ireland in the year 1885.^a

Towards the close of the year 1886, I met with another instance of true relapse in enteric fever, in the person of a member of the medical profession—a gentleman aged thirty-two years. The primary attack lasted exactly 28 days, and was followed by a week of apyrexia, during which the bowels were confined. On the thirty-fifth day rigors occurred in the afternoon, and temperature rose to 104° within 30 hours. This new fever lasted 14 days, and was accompanied by a fresh but scanty crop of rose-spots from the seventh day onwards.

The third case is the subject of the present paper. In my communication to the Academy in 1885, I pointed out that among the many causes of renewed pyrexia, or feverishness, in the later stages of enteric fever, or in convalescence from this disease, true relapse necessarily occupies a foremost place. And this arises, not so much from any increased danger to the patient's life—which is theoretical rather than founded on fact—as from the comparative infrequency of the occurrence of true relapse in this form of continued fever.

On that occasion I also stated that by “true relapse” I understand a second attack, in which the characteristic phenomena of enteric fever present themselves in sufficient number to establish the diagnosis of the disease—for example, enlargement of the spleen, abdominal tenderness, ochrey diarrhœa, and rose spots; or epistaxis, feverishness with evening exacerbations, abdominal tenderness and tympanites;

^a Trans. of the Acad. Med. in Ireland. Vol. IV. 1886. Page 1. See also Dub. Journ. Med. Science. Vol. LXXX. December, 1885. Page 486.

or any other grouping of the symptoms of this fever met with in practice; the fact being admitted that a perfectly typical case of primary enteric fever, showing *all* the characters of the disease, does not often come under observation, even in the wards of a large epidemic hospital. "By a relapse of enteric fever," writes Murchison,^a "is understood a second evolution of the specific febrile process, after convalescence from the first attack is fairly established. Relapses must not be confounded with recrudescences, which are common during the stage of ulceration." It is interesting to observe that, as usual, the relapse in the case I have detailed ran a shorter and on the whole a milder course than the primary attack. This is in accordance with clinical experience. Murchison states that the duration of the second attack is commonly, but not necessarily shorter than that of the first, while its type also is generally milder. As a result of his investigations, it would appear that a relapse is only about half as dangerous to life as a first attack.

The ætiology of relapse in enteric fever is not easily explained. In my former communication to the Academy on the subject, I have fully stated the views which are held upon this point. In the instance now before us, I cannot help thinking that the patient was poisoned afresh with the virus of enteric fever from without while he was in a particularly susceptible condition, in part owing to his weakened state, in part owing to the obstinate constipation which characterised in his case—as in so many others—the stage of convalescence.

^aThe Continued Fevers of Great Britain. Third Edition. Edited by W. Cayley, M.D., F.R.C.P. London: Longmans, Green & Co. 1884. Page 552.

Discussion.

The adjourned discussion on Dr. M. A. Boyd's paper on "Some Recent Modifications in our Views of Enteric Fever and its Treatment," which was read at the last meeting of the Section, was then opened in connection with a discussion on the papers which had just been read.

The PRESIDENT criticised one or two points in the clinical history of the case described by Dr. Moore, which occurred to him to need explanation. First, with reference to the significance of the diazobenzole reaction. In his own experience—and he had applied the test almost daily to a large number of cases in Sir Patrick Dun's Hospital—he did not rely upon it as by any means a safe clinical test, either as to diagnosis or prognosis in fever, and he did not agree with Dr. Moore that the failure of the test was due in any way to the fact that his patient had had fever fifteen years before. Secondly, he asked further information as to the clinical means of recognising the very remarkable fact he recorded of the heart, in early and marked weakness—viz., having "sagged from side to side" with change of posture, when in his experience and according to the teaching of the late Dr. Stokes, the impulse of the heart in cases of typhous softening cannot be felt at all in these cases, or at best with the greatest difficulty.

DR. H. T. BEWLEY did not think the question as to relapses in typhoid could be solved until the whole question of immunity, natural and acquired, was more certainly known than it is at present. We do not know accurately why cases of infective fevers got well at all; possibly the greater tendency to relapse in typhoid fever as compared with such other fevers as typhus, scarlatina, and measles, was connected in some way with the long and irregular course, and prolonged termination of the fever. With regard to giving solid food, there are some cases of typhoid in which the fever will not come down, and the patient grows weaker until some solid food is given. It is a very difficult question to say exactly when the time has come to give this solid food, and he would look anxiously for further information on this subject. With regard to diet, he did not think one could be at all dogmatic, seeing that the practice of skilful and experienced physicians differed so much as to the amount of food they allowed.

DR. WALTER SMITH said that Dr. W. Beatty's paper was essentially a qualification or limitation of Graves' celebrated maxim.

But if it is possible to under-feed fevers, it is also certainly possible to over-feed them, and so to hurt the patient. Obstinate vomiting, for example, may be caused by too zealous administration of milk. The condition of the tongue is a more satisfactory guide as to convalescence than the temperature or pulse, and two weeks, as a rule, should be allowed of normal evening temperature before any liberties are granted to the patient. The diazo-benzole test applied to the urine of enteric fever is unreliable and unsatisfactory. It is merely a curious colour-test for a chromogen in the urine.

DR. LITTLE considered there could be no doubt of the value of a restricted diet in enteric fever. There could be no doubt of the value of milk when diarrhoea is present; but some patients are unable to digest milk, and especially if then danger arises from cardiac failure, he considered properly made animal broths more suitable than quantities of milk. Though a matter which had only cropped up incidentally, as Dr. Moore had mentioned the use of quinine in his case of enteric fever, Dr. Little was tempted to ask why it was that quinine was so generally given in enteric fever? As far as he had seen no good effect was produced by quinine in enteric fever, and sometimes headache, sleeplessness, and intestinal disturbance resulted from its use.

DR. BOYD, in his reply to the criticisms on his paper on the antiseptic method of treating enteric fever, considered that so far no more rational method of dealing with the disease from its pathology presented itself, and notwithstanding Dr. Smith's doubts as to any disinfectant reaching the intestine in sufficient strength to be of use after passing through both stomach and duodenum, he thought if any form of antiseptic could survive unaltered, the gaseous one should—especially in a solution which slowly parts with it. Chlorine evolved from an alkaline solution—such as the liq. sodæ chlorinatæ of B. Ph.—in his hands was followed by better results than when formed in an acid mixture. As regards the relapses in enteric fever, they seemed to him to be due in most instances to a fresh set of glands and Peyer's patches becoming the seat of fresh infection by typhoid bacilli. He lately showed an instance, in a pathological specimen exhibited at the last meeting, of a portion of the ileum, the seat of active ulceration in a patient who was ten days apyretic, when he died from acute perforation, the result of over-eating. This specimen showed how long a condition of ulceration may exist after typhoid, even when the temperature has fallen permanently for as long as a week.

DR. WALLACE BEATTY, in reply, said that he quite agreed with Dr. Bewley and Dr. James Little, that exceptional cases of enteric fever occur in which the evening temperature remains above the normal long after all the other symptoms of enteric fever are over, and where no discoverable complication is present. In these exceptional cases he agreed with Dr. Bewley and Dr. Little that solid food cannot be withheld.

DR. J. W. MOORE, in reply, agreed with the President and Dr. Walter Smith that the diazo reaction was open to doubt as a diagnostic of enteric fever; nevertheless, it was an interesting, and often a confirmatory, test of the presence of this fever. He did not depend solely upon palpation as a method of physical examination of the heart, as the President seemed to think, but both auscultation and percussion indicated undue mobility of the heart in his patient's case. In answer to Dr. Little, Dr. Moore said that he sometimes gave small doses of quinine in enteric fever—not to reduce temperature, for fulfilling which object large doses alone would avail—but as a tonic, from his experience of the remedy in other acute and prostrating maladies. Once the specific origin of enteric fever was admitted, Dr. Moore believed that no mere error in diet would bring about a true relapse, although it may be a contributory cause of such an accident.

CASE OF STRICTURE OF SIGMOID FLEXURE OF THE COLON.

By J. P. DOYLE, L.R.C.P.;

Surgeon to the Hospital for Children.

[Read in the Section of Medicine, March 11, 1892.]

A MARRIED man, forty-five years of age, came under my care in October, 1889, complaining of soreness about the anus, tenesmus with passage of mucus, a feeling as if he were sitting on a plug, and pains in the abdomen and lumbar region. For a considerable time he had suffered from habitual constipation, and for the previous six months the bowels did not act without purgatives, which were followed at times by a little diarrhoea. He had great trouble from the motions being very tenacious. There was tenderness on pressure over the left iliac and lumbar regions, and the abdomen was hard. Examination per anum caused great pain, the sphincters were very contracted, the rectum was empty but felt swollen, the heart and lungs were normal, pulse regular, 64; temp. 97° Fahr. No albumen or tube-casts in urine. On November the 5th, as he had not a motion for some days, and abdomen was distended and painful, I gave him an enema for the first time, but he could not retain more than six or eight ounces of the fluid, which was returned immediately along with some opaque gelatinous mucus. Pains in abdomen frequently set in after dinner, and on December 19 they were so severe that he had a weakness, attended with convulsive movements. Before the bowels acted he had griping and colicky pains, with a dull aching about the lower part of centre of lumbar

and sacral regions. After a motion, for about two hours, there were pains and soreness in the abdomen; at times he passed quantities of red or clear gelatinous mucus mixed with or adherent to the faecal matter. On January 5, 1890, I could distinctly make out the descending colon and sigmoid flexure distended; they were very tender on pressure. Shortly after my examination he had two large evacuations, and on the following day all hardness and tenderness had disappeared. On March 27 I gave him a long enema tube and taught him its use. On April 3, after a walk, he got an attack of diarrhoea; and again, on May 13, he felt very ill, his bowels being very free. The integument of the lower part of the left side of his abdomen had become, from constant hot applications, of a dark mottled brown colour. He had been suffering very much from flatulence, and found from experience that milk diet suited him best. Between Oct. 15, 1890, and April 16, 1891, he progressed favourably, all his symptoms being very much improved, and he was able to pass the enema tube with greater ease. On April 18 he had an attack of obstruction with peritonitis; and again, on the 2nd of July, he had a similar attack, both produced by indiscretions in his diet. On July 8 passed some blood and mucus after using enema. August 26, while on the commode after an enema, he felt a sudden pain in the anus, which extended upwards, and gradually over abdomen. When I saw him on the following day he had all the symptoms of ruptured intestine with obstruction, which was followed by peritonitis, of which he died on September 4.

This patient received great relief from opiates, baths, enemata of warm water, mixed at times with antiseptics; he frequently used 4 or 5 injections daily. Large doses of pulv. ipecac. were also of service. The character of the abdominal pains varied, being described as continuous,

colicky, or griping, and frequently there was a sensation as if a tight girdle was round epigastrium.

Post mortem. — The abdominal cavity contained some reddish-brown fluid, the peritoneum was inflamed, fresh lymph was thrown out over the intestines; the rectum, descending colon, and sigmoid flexure were bound down by old adhesions and agglutinated by recent ones to neighbouring portions of the viscera; parts of the colon and small intestine had an ecchymosed appearance. The rectum was empty; the mucous membrane, of which the greater portion looks very thin and smooth, and nearly devoid of rugæ, presenting the appearance very much like that described as being present in so-called ballooning of the rectum. The ileum, ascending, transverse, and descending colon contained fæces; the latter was distended and packed with them, as far as the stricture in the sigmoid flexure. The rupture had taken place at the position of the kink at the inner side of the intestine, and apparently below the narrowing, as the opening in the stricture, which would admit about a No. 10 catheter, could be seen, and bulging against it but not escaping into the peritoneal cavity, was a hardened mass of fæces; part of the strictured tissues were included in the rent. The outer part of the strictured tissues consists of a narrow hard band, and looks as if produced by the contraction of the cicatrix of an ulcer. The walls of the colon, for 8 or 10 inches above stricture, are in parts very much thickened, of a dark purplish-red colour, and the lumen is diminished. Signs of inflammatory changes are seen here and there as far up as the ileum.

The immediate cause of the rupture in this case is, I think, not common; it was produced by the over-distension of the intestine below the seat of narrowing with water after an enema used by the patient, as after injecting the bowel, and while sitting and straining on the stool the

rupture occurred. The rent in the intestine, when seen on opening the abdominal cavity, was principally below the stricture, and looked as if the tissues, which had filled up the kink in the sigmoid flexure, had been torn through. The hard faecal mass was pressed against the small opening in the stricture, but none escaped into the peritoneal cavity. The diagnosis of the conditions that existed in this case when it first came under my observation was by no means easy, and I could only treat symptoms until the nature of the lesions gradually evolved. The microscopical examination of the gelatinous mucus was of use in giving no evidence of a malignant nature. I found the examination of the rectum and colon by the long tube, while administering an enema, assist in determining the existence of stricture.

A CASE OF OPIUM-POISONING.

By JOHN J. BURGESS, F.R.C.S., L.R.C.P.;

Late Assistant Surgeon, Richmond Hospital, Dublin.

[Read in the Medical Section, Friday, March 11, 1892.]

WHEN we consider that opium in some form is the most frequently used drug in medical practice, and when we see, on the one hand, the idiosyncrasy which some have for its smallest dose, and, on the other, the facility with which the general public can procure from any chemist large quantities of the tincture, we must be astonished at the comparatively few cases of poisoning which are reported.

Laudanum is sold to any adult person by the apothecary, provided he gives a satisfactory explanation there and then of the use he intends to make of it. Although it is out of the province of this paper to enter into the legal question of the sale of poisons or to suggest a remedy, still I feel bound to allude to the practice as highly dangerous to society which puts into the hands of the dipsomaniac or melancholiac an instrument capable of producing such disastrous consequences.

The case I am about to bring before the notice of this Section is one of acute poisoning by opium, and I would not take up the time of the Academy were it not that there is something to be learned from the details of treatment in each instance of this kind, and that we may profit by each other's experiences in order to be prepared when called on, as, at any hour of the day or night, we may have to take up charge of a patient in a similar unhappy condition.

CASE.—A. B., a lady, aged thirty years, of good physique, but intemperate habits, was observed by a servant to drink a two-ounce

measure-glassful of laudanum at 11 p.m. on the night of the 18th of December, 1887. The servant informed the husband a few minutes afterwards. He was disinclined at first to believe the statement, but about thirty minutes after the supposed time of his wife's swallowing the laudanum, seeing she became drowsy, he called in the assistance of a medical student who was living in the house.

I now insert the account of this gentleman:—

“My attention was called to A. B., on being informed that she had drunk two ounces of laudanum. I found her in the following condition—a marked condition of drowsiness with contracted pupils. Although I could see from her appearance she had evidently taken the drug to some extent, I was not inclined to believe she had taken so large a quantity as the servant represented. I got some strong tea prepared and made her swallow, after some persuasion, about half a pint of the infusion. A. B. was at this time walking about in a somewhat dazed condition. After taking the tea she retired to bed, but, not feeling easy regarding her, I remained in my room reading. About 1 a.m. the husband came down in a state of alarm, saying she was in a deep sleep, from which he had been unable to arouse her by shaking or calling her by name. I found her lying, as he described, in a condition of sleep, with slow and stertorous breathing. After some difficulty I succeeded in arousing her, but on discontinuing my efforts she again sank into the same lethargic condition. When aroused she was just able to say, ‘Please, let me sleep.’ Her breath smelled of laudanum. Her pupils were contracted to an extreme degree, giving the eyes a peculiar appearance. With her husband's assistance, each of us taking an arm, we forced the patient to walk up and down the room. I endeavoured to make her swallow some ether, but without avail. At last, seeing she grew worse in spite of my efforts, and that her husband, who had been assisting me in the attempt to arouse her, had now completely broken down, thus leaving me to my own resources, I sent him for medical assistance, while I continued to force her up and down the room, dragging her the greater part of the time, despite which she rapidly sank into a comatose condition.”

I saw the lady at 3 a.m. She was then in a collapsed condition. Efforts had been made to keep her walking, but it was at that time only possible to drag her across the room, as the power of motion seemed completely gone. She was still capable of

being roused by slapping her face and chest with a wet towel, but immediately sank again into a comatose sleep. Her pupils were veritable pinholes; her breath gasping; pulse imperceptible; surface of the body cold.

I had hardly entered the room when her breathing ceased. All our efforts could not arouse consciousness, and life appeared extinct. Seeing she was too far gone to use the stomach-pump, not having any atropin with me, I at once started artificial respiration, getting the medical gentleman who was there to inflate her lungs. We gave her hypodermics of ether and enemata of brandy and tea.

After two hours with her she became slightly conscious, was able to speak a few words, and to swallow some tea.

I then passed a catheter, and drew off a pint and a half of urine, which smelled of laudanum. This was, however, a brief respite. Although we did all we could, in a few minutes she again sank into a comatose condition. I then tried cold affusion to her head, and continued the artificial respiration, with hot jars to her feet, axillæ, thighs, and sinapisms to her legs, epigastrium, and other parts.

The enemata of brandy and tea with the hypodermics of ether were administered by one of us when the other was working the artificial respiration. Our efforts did not produce any effect; except now and then a gasp there was no sign of life.

At 7 a.m. I inserted hypodermically $\frac{1}{100}$ grain sulphate of atropin; a second injection fifteen minutes afterwards; a third in half an hour; and a fourth in thirty minutes later. Her condition before giving the atropin was—1. An attempt at respiration, due, I believe, to artificial means; 2. An imperceptible pulse; 3. A dry skin; 4. Pupils somewhat dilated, which was due to the failure of her vital power. These injections I firmly believe, as you shall see from the subsequent history, did no good.

She was seen at 9 a.m. by the late Dr. Corley and Professor Hamilton. The outlook was very bad. Dr. Corley was inclined to think there was no use continuing the treatment, but I determined as long as there was the least sign of life to do all I could, hoping that a certain amount of the poison would be eliminated by the kidneys if we could keep her alive long enough.

We continued the artificial respiration, and gave another $\frac{1}{100}$ grain of atropin at 12. The patient's condition then was the same; there was an occasional feeble effort at respiration.

At 1 p.m. I thought all was over; her lower jaw dropped; all

attempts at respiration ceased. I was on the point of giving up when I remembered I had sent for a magneto-electric machine early in the morning. Getting one pole over the back of the neck and the other on the frontal region, I set the current working. After what appeared a long time I heard a gasp, followed by a second. This gave me new hope, so I recommenced the artificial respiration. The current appeared to have set the respiratory apparatus working, as the feeble respirations continued until 2 p.m., when the same deathlike condition was present—the face, which was all along pale, became dusky; the eyes open with dilated pupils, and the lower jaw dropping, while all respiration had ceased.

After the last success with the electricity I did not despair, so putting one pole over the epigastrium, and the other to the nape of the neck, I turned on the current. The gasping respiration again commenced. Now getting my assistant to follow each effort with the arms raised over her head, I kept the current on for twenty minutes. At that time the feeble respirations became for the first time regular, four to the minute. Fearing lest the electrical stimulus might be exhausted, we again continued the artificial respiration alone. For two hours the respiratory efforts were very feeble, but regular.

At 4 p.m., greatly to our joy, they rose to 8 per minute, and became gradually stronger until 5 30, when, the breathing being 13 to the minute, the conjunctival reflex appeared, and artificial respiration, after fourteen hours, was discontinued.

Later on the patient was able to take nourishment; the pupils acting to light. I left her at 2 a.m. under the care of a nurse, with directions to awaken her every hour and give her some hot milk.

The further history of the case is of no interest. She suffered some trouble from abscesses after the ether injections; but when I saw her some weeks ago—nearly four years after that memorable night—she was in perfect health.

I wish to draw your attention in reference to the above case to the following points:—

As to the quantity taken, we found out afterwards it was three ounces—that is, 99 grains—of opium. This we discovered by comparing the known quantity of laudanum

which was in the bottle at 10 p.m. with what we found there in the morning, no one having access to the room where it was left in the meanwhile. The difference was about three ounces. This bears out the servant's statement as to having seen the patient drink a two-ounce measure full of laudanum. An ordinary two-ounce measure, when filled to the brim, contains about three and a half ounces.

I am aware that much larger doses of opium have been recovered from. In "Guy's Hospital Reports" it is mentioned that a patient took five ounces of laudanum without producing the slightest poisonous effects. Dr. Bowstead, of Wycombe Regis, reports a very remarkable case of recovery in a lady who had drunk eight ounces of laudanum and was not seen until fourteen hours after the poison had been taken, owing to her having locked herself in her bedroom. There are other cases reported of recoveries from similar quantities.

But when we come to cases in which the medical man who attended was able to ascertain for himself the amount taken without any source of error, and compare them with the former accounts in which hearsay evidence of the patient's friends was taken, we must be struck with the fatal results in comparatively small quantities.

Amongst many cases I will merely instance one recorded by the late Mr. Kirby, in which the fatal dose was one ounce; and a second, by Dr. Cleveland, of Maida Vale, where, although the young man was seen almost immediately after swallowing two ounces of Battley, his stomach washed out, and all the other aids tried, the case became rapidly fatal.

One peculiar thing about my case was that there was no vomiting, which generally follows so large a quantity of laudanum taken at once.

I may here remark the patient was, from all I could

ascertain, not given to taking the drug. She had once or twice had a draught of it, about 20 minims, prescribed for some dyspeptic trouble. So there is no doubt she took a very large poisonous dose, and was not previously tolerant of opium.

The next point is the secondary asphyxia of opium-poisoning which occurred here, you may remember, after two hours' artificial respiration. Six hours after the poison was taken she recovered consciousness and was able to speak, but in a few minutes sank again.

This is a condition I find referred to without any explanation being given in Taylor's book on Poisons; and on looking up the literature I find this was a very bad symptom; nearly all the cases in which it was present died. The late Mr. Kirby, of this city, mentioned the case of a lady who recovered after fourteen hours, was able to speak to those around her, but suddenly became unconscious, and died four hours afterwards.

I would venture to suggest that this depends on a secondary absorption. The first quantity taken into the system had a paralysing effect, so that when the condition improved a residue of the poison which remained in the stomach was taken up.

As to the treatment, there is no doubt that the stomach should have been washed out when the first symptoms appeared. It was the uncertainty of the quantity which kept the friends of the patient from sending at once for proper assistance. When I arrived it was too late, for three reasons:—

1. Her respiration had stopped.
2. Probably the greater part of the laudanum was absorbed.
3. The danger was from the opium in her blood, not from what remained unabsorbed.

Next as to cold douching; this, I believe, did more harm

than good. I would only employ it when there is a moderate case with warm skin, and not in a collapsed condition like the one in question.

The atropin was of no avail, and had no effect either on the heart or respiration. In fact, the respiration was worse after its use.

In my opinion this patient owes her life to three things:—

1. The artificial respiration continued for fourteen hours.
2. The interrupted current, without which the first would have, I believe, failed.
3. The hypodermics of ether and enemata, which latter, getting some fluid into the system, caused a certain amount of diuresis.

In conclusion, I would beg to point out, from the apparently hopeless nature of this case, that we ought to be very slow to give up a case of opium-poisoning if there is the faintest sign of life, and even when that appears extinct, should not discontinue our efforts for at least twenty minutes after apparent death.

DR. FALKINER gave an account of two cases of opium-poisoning which had come under his notice. In one a young man suffering from syphilis had drank about 5 oz. of a lotion containing 1.5 grains of the extract in each ounce, or 150 grains of crude opium. He was not under treatment until 13 hours after the poison was taken, and was not considered safe until 16 hours after the treatment commenced. The interrupted current was of great service. The second was a case of a lady who was suffering from severe neuralgic pains, and a uterine tumour, who received three hypodermic injections in the space of six hours of $\frac{1}{2}$ -grain morphin in each. The lessons to be drawn from these cases were that, in the first case, a practitioner had left it as hopeless before treatment was commenced. In the second case the hypodermic administration had been left in the hands of a non-professional person.

DR. JOYNT mentioned the case of a Parsee lady whom he had

been called to see some years ago in Surat, in India. The lady was about seventeen or eighteen years of age, and had swallowed about an ounce of Collis's chlorodyne. Before he saw her she had been treated by a Parsee medical graduate, who had employed all the usual remedies laid down in medical books—stomach-pump, electricity, flagellation, and artificial respiration, &c. After again in vain repeating some of these, and the comatose condition in which he found her progressing steadily, he had recourse to cold affusion, in the form of a stream of iced water slowly poured on the patient's head from a height of several feet. After a considerable time she responded to this treatment, which was carried out more or less continuously for about two hours, when she so far recovered as to be able to swallow a cup of strong coffee. Perfect recovery resulted. The poison had been swallowed about four hours before Dr. Joynt saw her.

DR. LITTLE considered that the expediency of relying on atropin as an antidote to opium-poisoning was very doubtful. It did not appear to have done good in Mr. Burgess' case. He thought the communication most valuable as showing we should not give up any case of opium-poisoning until life was extinct. The remedies which appear to have done most good were the persistent artificial respiration and external stimulation. In a case which he had treated many years ago life was saved apparently by keeping the patient awake by slapping various parts of the body exposed for the purpose with a spatula dipped in very hot water and the use of hot coffee by enema and by the mouth. He doubted the safety of using the faradic current to the head, but found it in the case he had himself treated useful when applied to external parts as a mode of waking up the patient.

DR. HASTINGS TWEEDY mentioned a case admitted into Steevens' Hospital two years ago, when he was house-surgeon there. His case, unlike the other cases cited, died, though the dose taken was less than that in Mr. Burgess' case. Having heard the very instructive case of recovery he could not attribute his non-success to anything but to the more orthodox methods he employed in comparison to Mr. Burgess. The stomach was washed out, ether was not injected, atropin was injected to a large extent. The patient was admitted at 3 o'clock in the afternoon. At Dr. Hayes' suggestion, they kept up artificial respiration until 10 o'clock the following morning. She was then breathing at the rate of 12 per

minute. She continued to breathe quietly until 2 o'clock, p.m., when she suddenly died 23 hours after she was admitted.

DR. HAYES said that, having seen the patient mentioned by Dr. H. Tweedy, he was strongly of opinion that the injection of atropin distinctly benefited the patient, and he thought that it was to be regretted that in that case the artificial respiration had not been longer persevered in.

DR. WALTER SMITH remarked upon the wide limits which separate the smallest fatal dose of morphin from the maximal dose, upon which recovery has followed, the ratio being 100-1. The use of atropin in the treatment of opium-poisoning is still an undetermined question; when it appears to do good it is probably by its action as a respiratory stimulant. The state of the respiration is a better test than the condition of the pupil. It is strange how conflicting are the views as to the excretion of morphin, an important point both in toxicology and forensic medicine. It seems to be now established that morphin is mainly excreted into the stomach and bowels, and so cast out in the fæces. Very little goes out in the urine.

DR. H. T. BEWLEY said he had seen a case of morphin-poisoning in which the breathing became exceedingly shallow and feeble, and gradually became slower until the respiratory movements occurred only about four times a minute. He injected atropin several times, about $\frac{1}{30}$ grain in all, after which the breathing became better and stronger, and increased in frequency up to 6 or 7 times a minute. The patient died—in fact, she was dying of liver disease at the time she took the morphin.

DR. FINNY, alluding to the question of the antagonism of the alkaloids of morphin and atropin, referred to a communication he had made on that subject before the Medical Society of the College of Physicians some twenty years ago, in which he illustrated—and as it seemed demonstratively—the important curative antagonism of morphin in a case of atropin-poisoning, while he questioned the reliability of the treatment of opium-poisoning by the hypodermic use of atropin.

MR. BURGESS, in reply, said he believed in cold douches, but not in a state of collapse, having often used them in milder cases of alcohol-poisoning with success. He confessed that strychnin injections did not occur to him until afterwards, but he had no doubt when the breathing became regular this would have done excellent

service. The atropin was not pushed any further because—(1) It had no effect on the respiration or heart during its application for over three hours. (2) He shrank from introducing a second poison into the system when he was not deriving any benefit from the quantity already given. He was glad to find that Dr. Smith and the President agreed with his views about atropin not being what it is represented to be, an antidote for opium.

THE DESIRABILITY OF OPERATIVE INTERFERENCE IN SUSPECTED PERFORATION OF CHRONIC ULCER OF THE STOMACH.

By ALFRED R. PARSONS, M.B. UNIV. DUBL. ;

Assistant Physician to Sir P. Dun's Hospital.

[Read in the Section of Medicine, May 6, 1892.]

PATHOLOGICAL observation and experimental investigation, particularly the latter, have done much to aid clinical diagnosis, and to render feasible operations which seemed little short of impious to our forefathers. The physician can often, from the clinical history and symptoms, now localise a cerebral abscess with sufficient certainty to justify a surgeon in applying his trephine and attempting to drain it. A clearer knowledge of the origin and functions of the spinal nerves has enabled the neurologist to point accurately to a certain level as the seat of spinal pressure, and the surgeon's knife has confirmed the accuracy of the diagnosis, and by the removal of the tumour made life a pleasure where it had been full of pain. Certain symptoms and signs referable to one hypochondriac region, and the presence in the urine of a small quantity of caseous pus, containing tubercle bacilli, justify, with certain restrictions, a recommendation of surgical interference, and the excision of a kidney with a localised tubercular focus has saved an otherwise forfeited life. Opening the peritoneal cavity demanded at one time no less an atonement than the life of its possessor; but to-day the surgeon, strong in his antiseptic precautions, does an exploratory laparotomy with greater coolness and confidence than he can find in the administration of chloroform to a child. But though much has been done in the past, and many lives have

been saved by the combined action of physician and surgeon, much still remains to be done, and I trust that the day is not far distant when the man who dallies with acute perforative peritonitis will be equally guilty with the practitioner who follows the hounds, knowing a patient of his is suffering from a strangulated hernia, and then calls in some one else to share the responsibility with him. To one of the causes of acute perforative peritonitis—namely, chronic ulcer of the stomach—I desire in this paper to refer; and I would wish, from my very limited experience, to attempt to deduce the phenomena which indicate the occurrence of perforation, and then suggest what seems to be the only line of treatment offering any hope of success. The following cases came under my notice last year, as House Surgeon in Sir P. Dun's Hospital, and my best thanks are due to the staff of the hospital for permission to record them, and for the assistance they gave me in looking up the literature bearing on this subject:—

CASE I.—Michael C., aged twenty-seven, a well-built, muscular man, was brought to hospital about 9 a.m. on Saturday, 28th March, in an almost unconscious condition. The history we obtained from his friends was, that he had been engaged at his usual work, and apparently in perfect health, till 12 noon on the previous day, when he was seized with violent abdominal pain, which doubled him in two. He went to bed, and as the pain did not subside, medical aid was called in. The doctor who saw him administered castor-oil and opium by the mouth, applied stupes to the abdomen, and gave an enema. His relatives were advised to have him moved to hospital the following morning, if there were not a decided improvement in his condition. During the afternoon he vomited some reddish-coloured fluid, which was thought to be blood. He passed rather a sleepless night, suffering considerable pain, but was able the following morning to assist in dressing himself, preparatory to removal to hospital. On admission he was cyanosed, pupils were dilated, sweating on forehead, pulse almost imperceptible, abdomen somewhat distended and very tympanitic.

Complete loss of liver dulness. It soon became impossible to feel the pulse at the wrist, and within fifteen minutes after admission respiration had ceased. In this case the physical signs, combined with the history, left little doubt that we were dealing with a perforation, and the diagnosis of acute peritonitis due to perforating ulcer of the stomach was almost quite accurate. At the *post-mortem* examination we found recent acute generalised peritonitis, evidently produced by the rupture of a small round ulcer situated just at the commencement of the duodenum. The other organs, as far as they were examined, appeared quite free from disease.

CASE II.—Miss M. G., aged twenty-one, was admitted to Sir P. Dun's Hospital about 10 30 p.m. on Saturday, the 28th November, 1891, suffering from intense abdominal pain. She told us that her general health, with the exception of occasional attacks of constipation, accompanied by pain, had been good till the onset of her present illness. She gave no account of vomiting blood or of pain after taking food. The history of her illness dated from Thursday, 25th November, though since the previous Sunday there had been no motion of the bowels, and she had had slight attacks of pain referred to the abdomen. These attacks, however, were not sufficiently severe to have prevented her from attending to her daily work. On Thursday, at 8 p.m., just two days before her admission to hospital, she was seized suddenly with an attack of violent abdominal pain, accompanied by vomiting. On the next day, as the pain was still very severe and the vomiting continued, the family physician was called in. He administered soap and water enemata, and glycerine enemata, but failed to move the bowels. A dose of castor-oil, which was not vomited, was likewise ineffective. On the following day (Saturday) Dr. Ball was asked to see the patient in consultation. He visited her in the afternoon and recommended her immediate removal to hospital. On admission some four hours later the patient, who was well nourished and developed, appeared to be suffering considerable pain. The cheeks were of a bluish red hue, but the lips, conjunctivæ, and gums were rather anæmic. The tongue was coated with white fur, and the breath was fœtid. Respirations were hurried (45 per minute), and the alæ nasi were working actively. The frequency of the pulse was likewise considerably increased—132 per minute, small in volume and feeble. Her heart and lungs showed no indication of organic disease

The temperature was 96° F. The abdomen was considerably distended, though not uniformly tympanitic; it was tender to the touch, but the pain was, if anything, referred rather to the left than to the right side. The area of hepatic dulness was reduced to about two fingers' breadth. After consultation it was decided to postpone operative measures till the following morning, in the hope that the patient would then have recovered from the shock consequent on moving her, and that the advantage of operating in daylight would more than counterbalance any risk attendant on a delay of 9 or 10 hours. She had some morphia during the night, and the following morning, though her condition was very critical, it seemed that an operation would afford her the only chance of recovery. It was thought probable, from the history of the case, and particularly from the fact that the perforation—if perforation it were—must have occurred some 56 or 60 hours previously, that the lesion was connected with the vermiform appendix. The abdomen was therefore opened in the middle line, between the umbilicus and symphysis pubis. All the indications of recent acute peritonitis were present, but a careful examination of the vermiform appendix and pelvic viscera failed to disclose the cause. Dr. Ball then proceeded to explore the upper part of the abdomen, but finding the transverse colon perfectly free from any inflammation, concluded that the source of the mischief could not lie in that direction. The cavity was accordingly flushed out with warm water, and a drainage tube was inserted. The patient died that evening at 6 o'clock. A *post-mortem* examination disclosed the presence of a perforating ulcer, situated about the middle of the anterior wall of the stomach. One of the most unusual features in this case is the length she survived the perforation. Hilton Fagge says that if the patient live more than one day after the onset of perforative peritonitis, the probabilities point strongly to a perforation of the vermiform appendix, as nearly all the cases of ruptured ulcer of the stomach are fatal in less than 24 hours.

CASE III.—About 9 15 a.m. on Monday, 21st December, 1891, I was asked to see a lady who had been a patient in the hospital on three or four occasions during the years 1890 and 1891, suffering from attacks of severe pain referred to the stomach. These attacks were not very closely related to meal times, as they occurred sometimes before, sometimes after, and often quite independently of partaking of food. She never vomited any blood. Conse-

quently, though gastric ulcer, amongst other possible diagnoses, presented itself to the minds of those under whose care she was, I am not aware that any absolute diagnosis had been made in her case. These attacks of pain had often been greatly relieved by taking 10 grs. of antipyrin. I heard she had returned from her professional duties only the previous evening, and learned from herself that four days previously, an attack of pain more severe than usual had set in. Though she continued at her work she was, owing to the severity of the pain, able to take but little food during this period. She returned to town on Sunday evening, but could not sleep till 6 o'clock the following morning. She slept then for some two hours, and awoke a little after 8 in most violent pain. I was accordingly asked to see her, and found her about 9 30 a.m. still suffering considerably. She referred the pain chiefly to the left hypochondriac region. The pulse was 100 per minute, respirations were tolerably deep. An examination of the region to which the pain was referred failed to detect anything abnormal. On auscultation the heart sounds seemed unaccountably feeble. I regret to say that I failed to recognise the nature of this case at once, because I did not grasp the fact that she had had a sudden onset of very violent pain at 8 a.m., and I understood from her that the pain was not quite so bad as it had been a little previously. I recommended the application of stupes to the side, and administered a small quantity of a stimulating carminative mixture. However, on thinking over the case it struck me that very probably it was a perforation that had taken place, and I was confirmed in this opinion when I saw her an hour or so later; for at 11 a.m. the collapse was much more marked, the frequency of the pulse had increased to 110, finger nails were blue, face was drawn and anxious, and notwithstanding application of stupes and hot bottles patient was rather cold. Dr. Finny saw her shortly afterwards, and it was decided to remove her to hospital. A hypodermic of morphia and a stimulating enema were administered by his directions, and soon after her admission she rallied considerably, but the frequency of her pulse had increased to 120. On examination the area of hepatic dulness was found to be diminished, and the note over the hepatic region abnormally tympanitic. A catheter was passed and several ounces of urine free from albumen and indican were drawn off. A soap and water enema removed a small quantity of faecal matter. By Dr. Finny's order a consultation of the staff to decide on the advisableness of operative measures was summoned

for 4. By this time the pulse had increased somewhat more in frequency, but otherwise there was no marked change in her condition. After consultation it was decided to open the abdomen. Dr. Ball made the incision in the middle line above the umbilicus, and the moment the peritoneum was incised an escape of gas confirmed the diagnosis of a perforation, the site of which was found to be the anterior wall of the stomach, in its lesser curvature near its junction with the œsophagus.

The wall of the stomach all round the perforation was thickened, swollen, and so soft that sutures at once cut through it. The external circumference of the stomach at this part appeared to be so small that no hope could be entertained of excising the ulcer completely without leaving too great a constriction. Nothing could, therefore, be done except, by means of sutures passed through the healthy tissue, to draw the stomach up to the edge of the abdominal wound and carefully stitch them together. The peritoneal cavity was then washed out, and the lower part of the abdominal incision closed, leaving a gastric fistula. After the operation the pain in the abdomen ceased, she vomited only seldom, suffered little inconvenience from the wound beyond that due to a very profuse flow of highly acid gastric secretion, which irritated the skin for some distance round it, and may in some degree be answerable for the fatal issue.

Into the further details of this case it is not necessary to enter. Suffice it to say that she rallied well, and continued to improve till Wednesday, when a change for the worse set in, and she gradually sank till death occurred at 5 50 on Sunday morning—just six days after the perforation had taken place. A more unfavourable site for a perforating gastric ulcer can scarcely be imagined. Had it been more fortunately situated, the diseased tissue could have been easily excised, the opening closed with Lembert's suture, and the stomach returned to the abdominal cavity.

From these cases we may, I think, draw the more usual symptoms of perforation. In only one case was the previous history of any assistance to us in arriving at a diagnosis; while the other two presented no symptoms before their illness which could have aroused our suspicions of an internal ulcerative process. In diagnosing a perforation of the stomach, we have to rely mainly on the sudden onset of very

violent pain, often described by the patient as doubling him in two, the accompanying collapse, pallor, and anxious expression of the face, a pulse small in volume, compressible and steadily increasing in frequency; vomiting is also often, though not invariably, present; and the respiration will probably be rapid, chiefly thoracic, and productive of pain on deep inspiration. If we see the patient a few hours later, the severity of the pain may be slightly abated, the collapse not quite so marked, and the colour improved, but the frequency of the pulse has increased from 90 or 100 to 110 or 120, probably diminished volume and augmented compressibility. Visiting our patient some hours later—say 12 or 14 after the perforation has taken place—he expresses himself as much better; he has no longer the intense pain from which he suffered earlier in the day; there is no impairment whatever of his intellectual faculties; but, on the contrary, he is quite clear and collected, and looks to you to confirm the favourable opinion he has formed of his own condition. But, as you take his clammy hand in yours, and try to count the pulse, now barely perceptible at the wrist, as you feel the cold extremities, and see the sweat gathering on the pallid countenance, you read the words “No hope!” written clearly on every feature. The prognosis is soon equally evident to the untrained eye. Restlessness comes on, slight delirium sets in, the pulse can no longer be felt, respiration becomes quick, shallow, irregular, slow, and finally ceases, in the majority of cases, in from 12 to 24 hours after the perforation took place.

Such I take to be a fairly typical history of a case of perforative peritonitis, whether it be left to nature or treated as such cases are generally treated. And we have now to ascertain what light does the *post-mortem* examination afford us? is the pathological condition, if persistent, necessarily followed by this sudden change from apparent health to

death? and, if so, are there no means by which this condition can be removed or modified? At the autopsy we find a body well developed, well nourished; on external inspection disclosing nothing except, possibly, some slight distension of the abdomen; the brain and its membranes are apparently perfectly healthy; detailed examination of the thoracic viscera discloses no disease; in the abdomen nothing morbid, except an acute diffuse peritonitis, evidently owing its origin to an extravasation of the contents of the stomach through a perforation in its wall. Must we not, as we stand by the opened body on the *post-mortem* table, feel humiliated that lesions such as these, situated not in the pons, medulla, internal capsule, heart, or any other region sacred against the intrusion of the surgeon's knife, but in the stomach—an organ which in man has been incised frequently, partly excised, and in the lower animals completely removed with success—should have been the cause of death? In this the age of gastrectomies, gastrostomies, gastro-enterostomies, pylorotomies, are these specimens not a reproach to our diagnostic skill, or operative courage? And must that reproach not grow deeper as we gaze upon the well-built body, not emaciated by prolonged fever or mal-nutrition, and examine the remaining viscera, not affected by any trace of disease, and fail to find in all anything but that small ulcer inconsistent with a prolonged and vigorous life?

But, it may be asked, is death, in the absence of operative measures, a necessary consequence to general peritonitis produced by the rupture of a gastric ulcer? To this I may briefly reply—Death is, in such cases, practically speaking, an inevitable result. I have examined carefully the literature of this subject, and can find recorded only 9 cases which presented symptoms indicative of a perforation of a gastric ulcer and recovered. Of these, 3 died subsequently from this affection, but in only one of them did the *post-mortem*

examination seem to confirm the original diagnosis. This case is reported by Hughes, Ray, and Hilton in "Guy's Hospital Reports" for 1846:—"A servant girl was suddenly seized with all the symptoms of perforation. Fortunately she had eaten nothing for four hours before this, and then only gruel. She was put under the influence of opium, was kept in the recumbent posture, and was fed by the rectum. She was discharged apparently cured after 52 days. Two months afterwards she was again suddenly seized with the same symptoms, and she died in four hours. The autopsy showed, in addition to a recent peritonitis, evidence of old peritonitis. There were adhesions of the coils of intestines with each other, and between the stomach and adjacent viscera. In the stomach were found a cicatrix and two open ulcers, one of which had perforated."

Such a record out of the many fatal cases of gastric ulcer which have been reported, fully justifies Mr. Treves when he says, "The lover of the curious will search long before he can find in the literature of perforative peritonitis the account of a well-authenticated recovery without operative interference;" and gives, for all practical purposes, a direct negative to the inquiry in such cases—Is there any hope?

If the prognosis, then, in these cases when left to nature or as usually treated be hopeless, can we do nothing when called to a patient presenting the symptoms I have enumerated? It seems to me that our duty under such circumstances is threefold:—1. To avoid increasing the mischief already done. Consequently all food, medicine, and stimulants by the mouth should be strictly prohibited. 2. To relieve the agony which the patient is suffering by a hypodermic of morphia, and to combat the collapse by the administration of stimulants per rectum. 3. To recognise *early* that the case before us is one imperatively demanding immediate operative interference. This appears to me,

under such circumstances, to be the highest function of the physician's art, and while sometimes the diagnosis may be quite evident, in others it requires great care to form, and courage to express, such an opinion. It is not to be expected that a physician should, as it were, see through the abdominal wall and accurately describe the nature and seat of the lesion; but ought he not to recognise that he has to do with some urgent intra-abdominal lesion imperilling his patient's life, and probably connected with the rupture of a hollow viscus, and that the only way in which the diagnosis can be completed and the patient's life saved is by an exploratory laparotomy? But the sceptic will probably ask, what has an abdominal section ever done for perforative peritonitis due to rupture of a gastric ulcer? I regret I can only point to a life prolonged a few days and to a less painful death in the cases I record in this paper; and I am free to admit that I have searched the literature of this question without finding one successful case. The explanation of this, I believe, is not far to seek. But first let me briefly mention what has been done in conditions somewhat resembling those under consideration.

In "St. Bartholomew's Hospital Reports" for 1873, Mr. Thomas Smith gives an account of several cases of general peritonitis consequent on rupture of an ovarian cyst in which he operated successfully. Professor Kocher did a laparotomy three hours after the receipt of a pistol-shot wound of the stomach, closed it with sutures, and the patient recovered completely. Dr. Ball's successful case of a boy, who received a stab in the abdominal wall penetrating the stomach, in which, four hours after the injury, he did a laparotomy, and stitched the edges of the wound together, will be in the recollection of some members of the Section. In a paper read before the Medico-Chirurgical Society of London in 1885, Mr. Treves described a case of acute general purulent

peritonitis due to the bursting of a pelvic abscess into the abdominal cavity, in which he opened the abdomen, flushed out the cavity, and put in a drainage-tube with the most satisfactory results. Several cases of successful laparotomies for general peritonitis secondary to perforation of the vermiform appendix have been recorded; and lastly, from Dr. J. W. Moore's recent work on the "Continued and Eruptive Fevers," I learn that of 19 attempted laparotomies for perforation in typhoid fever, 4 were successful. Handicapped as the operators in these latter cases must have been by extensive ulceration and inflammation of the intestine, by high fever—by, in fact, almost every condition that could militate against satisfactory union—such results were brilliant. Why have the cases of gastric perforation in the past not been equally successful? Chiefly because they have been done too late. They have been, in many cases, I fear, postponed hour by hour till the diagnosis was absolutely certain, and the patient almost moribund, lest it might be said, "The abdomen was opened and nothing found;" while in typhoid fever the onset of peritonitic symptoms was more or less expected, and no time was lost in deciding as to the advisableness of operating.

Two courses are open to us in treating cases of suspected perforation. We may reject operative interference, take our stand on the traditions hallowed by time and authority, and follow Heister, who, writing in 1739 on perforation of the bowel, could only advise that the patient be kept quiet, that he be urged to eat abstemiously, and to lie upon his belly, and that the rest be left to Divine Providence and the strength of the constitution; or, mindful of the recent great advances in abdominal surgery, of the very slight risk attending an exploratory laparotomy, of the fatal consequences of a perforation when left alone, we may make up our minds EARLY that the case calls for an abdominal section

In 1881 Dr. Marion Sims, when addressing a medical society in America, having described the sudden death of an eminent physician in six hours from intestinal perforation, and recommended opening the abdomen and stitching up the wound, said—"Rest assured that the day will come, and it is not far off, when an accurate diagnosis in such cases, followed by prompt action, will save life that must otherwise quickly ebb away." If that day is hastened in any degree by any statements I have made in this paper—if I have persuaded any of my audience to add acute perforative peritonitis to the generally-admitted four great surgical emergencies requiring immediate operative interference, my object in writing this paper is gained.

I must apologise for the trial to which I have subjected you with these rambling thoughts and quotations, and express my gratitude to you for the patience with which you have listened to me. The matter and manner of the communication are far from what I would wish them to be. The only excuse I can offer is, that as I stood by the bedside of some of these patients, and saw the vital tide fast ebbing away, if one thought were impressed upon me more than another it was, "To save such cases one must open the abdomen and open it **EARLY**."

MR. M'ARDLE said that Dr. Parsons must not contrast operations for injury, pelvic abscess, or ruptured ovarian cyst, with that for chronic perforations, since in these cases (as in one of those mentioned in this paper) the margin of the perforation is so soft that sutures will not hold, while in those cases of injury the edges of the wound are healthy and permit of suture. The other cases do not interfere materially with vital organs, and so should not be brought into the question. It is doubtful if Lembert's suture will ever be found effectual in closing perforating ulcers of the stomach, and the delay occasioned by the application of this suture would in itself prove fatal to a patient already in a state of collapse. It

seemed to the speaker that the introduction of a large bone plate through the stomach wound, sutures from this plate through the stomach wall, and out through the anterior abdominal wall, would secure the stomach forwards, thus establishing a fistula. This operation would occupy from 20 to 30 minutes, as in similar operations on the intestines, while resection of the edges of the ulcer and suture after Lembert's method must occupy a time sufficient to prevent a favourable result. Should we succeed in reducing the time required for the operation, and should the cases come under treatment before exhaustion or sepsis occurs, such success may be hoped for as attends laparotomy for other than perforative peritonitis.

DR. O'CARROLL agreed that, in cases which can be certainly diagnosticated as cases of perforation of stomach, operation is desirable. But it was inevitable that a large number of abdominal sections would be done without finding the perforation, and still more without being able to do anything of advantage to the patient. There were difficulties in diagnosis which had yet to be got over—namely, the recognition of perforation in cases in which a false stomach has been formed by peritoneal adhesions, and in which the fatal perforation has occurred, and the differentiation of gastric perforation from many cases of acute and subacute gastritis simulating it. Of course there remains the wider difficulty of being sure that the perforation is a gastric one at all.

DR. WALTER SMITH alluded to the grave responsibility which now so often devolves upon the physician of advising for or against operative interference in cases of acute intra-abdominal inflammation. Speaking from his own experience, he could confidently say that in no single case, even when the issue was fatal, had he reason to regret having called for surgical intervention.

DR. J. W. MOORE agreed with Dr. Walter Smith in his views as to the interdependence of medicine and surgery, but was glad to observe that, while thus expressing himself, Dr. Smith was careful to avoid any suggestion that a physician should practise surgery or a surgeon medicine. Dr. Moore recalled how closely the subjective sensations of improvement experienced by the victim of perforation after some hours resembled the mental state, amounting to euthanasia, often observed in those dying of malignant smallpox of the purpuric or hæmorrhagic variety. The success of laparotomy in cases of perforation in enteric fever was very encouraging. Up to the close of November 1891, 19 cases with 4 recoveries had been recorded. This represented a percentage of recovery of more than

20 per cent—no small gain in the case of the most perilous of all the complications of enteric fever.

DR. C. F. MOORE could not say whether present experience throws any light in the way of comparative pathology on the question of peritonitis from injuries or disease. He had once examined the abdomen of a ten-foot crocodile and found there several stones encapsuled in fibrous tissue and hanging thereby from different parts of the alimentary canal. Some of these foreign bodies were as large as a small closed fist, and had evidently been extruded without injuring the health of the animal.

DR. PARSONS replied.

THE LIMITS OF MEDICINE AND SURGERY IN DISEASE OF THE VERMIFORM APPENDIX.

By CHARLES B. BALL, M.D., F.R.C.S.;

Surgeon to Sir Patrick Dun's Hospital.

[Read in the Section of Medicine, May 27, 1892.]

UNDER the names of perityphlitis and paratyphlitis, we are all familiar with inflammatory conditions of the ileo-cæcal region, but the researches mainly of the American surgeons have demonstrated that it is rarely these inflammations originate in the cæcum proper, but are due to pathological changes in the vermiform appendix. McBurney, who has done so much on this subject, puts down the proportion as 99 of the latter to 1 of the former, while other writers of large experience deny the existence of a cæcal origin for the inflammation altogether; hence the Committee of the New York Medical Society have adopted the word Appendicitis, which, although not free from objection, is preferable as expressing a more accurate pathological significance. It is within the experience of us all that many of these cases get well under purely medical treatment, while on the other hand it is certain that many valuable lives are sacrificed which might have been saved by timely surgical operation; and it has occurred to me that a discussion on the limits of medical and surgical treatment might prove interesting, and be peculiarly suitable to the Medical Section of the Royal Academy. In the following remarks I will try and avoid going into purely surgical details, and will only adduce cases so far as is necessary to elucidate the subject. At the outset, I would at once disclaim against viewing the vermiform appendix with that extirpating energy which some modern

gynæcologists appear to exhibit towards the ovary on the very slightest pretext; on the contrary, I believe strongly that the majority of cases of appendicitis recover well under purely medical treatment; but at the same time it must never be forgotten that even in the apparently most trivial cases, urgent symptoms may at any moment become prominent, and that surgery, to be of use, must be resorted to at an early date. To leave the case until septic peritonitis has spread all over the abdomen, and until extensive meteorism has supervened from paralysis of the gut, is to consign the patient to almost certain death, as, if under these circumstances operation is undertaken, it will almost certainly be fatal, and has been well described as a premature autopsy.

The pathological processes which originate this inflammatory state are subject to some variety. In a large proportion of cases it will be found that there is present in the appendix a foreign body—this may be a fruit stone, grain of shot, or other similar substance which has escaped into the appendix from the cæcum, or an enterolith which has formed *in situ* by the concentric deposit of faecal matter and mixed phosphates. In either case the pressure of the body may produce necrosis of the epithelial lining of the appendix, and thus allow the septic bacteria, which are so numerous in the fæces, to escape into the submucosa, and give rise to ulceration or gangrene of the appendix. There is great anatomical variability in the orifice leading from the cæcum into the appendix; in some it is well protected by a valvular fold of mucous membrane, while in others it is patulous and the valve entirely wanting. Possibly this patulous state of the orifice, which is probably congenital, accounts in some degree for the fact that this disease is one occurring in early life in the vast majority of instances. In many cases, however, no foreign body or enterolith can be found, and the cause of the initial ulceration must remain doubtful. When an ulcer or gangrene of the

appendix takes place, it may be followed by plastic inflammation of the surrounding peritoneum without causing suppuration. This is what takes place in the milder cases which resolve spontaneously. In a second class of cases localised suppuration takes place, and an abscess forms which may point in the lumbar region or iliac fossa, while occasionally an intra-abdominal abscess may form, sometimes of very considerable size. In these cases the size may increase very rapidly by the development of gas, although the quantity of pus is small. There has been much contention as to whether these abscesses are intra- or extra-peritoneal; it will, however, be obvious that as the vermiform appendix is entirely surrounded with peritoneum, they must be, in the first instance, intra-peritoneal. But the distinction is not of great practical importance, because when once adhesions have taken place between the coils of intestine in the immediate vicinity and the parietal peritoneum, the case resembles one in which an abscess has arisen outside the serous sac, the great bulk of the cavity remaining uninvolved, but in all probability the danger of rupture into the general cavity of the peritoneum is greater where the wall of the abscess consists of adhesions only.

Where the floor of the ulcer suddenly gives way without the previous formation of limiting adhesions, extravasation of fæces will take place and septic peritonitis be the inevitable result, the acuteness of the peritonitis, however, being usually less than when the stomach or small intestine is perforated, death not usually resulting in the former until the fourth or fifth day. Mynter has suggested as a reason for this, that when the small intestine is perforated, the movement of the gut in peristalsis distributes the septic matter rapidly over a large surface of the peritoneum, while the more fixed position of the cæcum limits the peristaltic excursions of the vermiform appendix. A more probable

explanation, however, is that the very small calibre of the appendix as compared with the other portions of the intestinal tract permits of only a much slower extravasation of fæces. Among the more distant results of ulceration of the appendix may be mentioned the cicatricial closure of the lumen and the dilatation of the distal portion into a retention cyst. In one of the cases recorded by McBurney such was found to be the case. A cyst so formed might, of course, at any time rupture into the peritoneum, but it is not probable that the results would be so disastrous as in primary perforation of the appendix, the fæces not being extravasated.

From the foregoing it is obvious that three clinical types of appendicitis may be recognised, and the classification of Prof. With of Copenhagen is a convenient one:—

1. Peritonitis appendicularis adhæsiva.
2. Peritonitis appendicularis localis.
3. Peritonitis appendicularis universalis.

The symptoms by which the first variety is ushered in are—Sudden severe abdominal pain, usually more marked on the right side, vomiting, and a considerable degree of fever. An examination of the abdomen shows that the muscles of the right side are more rigid than those of the left, and McBurney has pointed out the fact that although the entire abdomen may be painful and sensitive to the touch, one point of extreme tenderness can usually be made out if carefully examined for. For this purpose the tip of one finger only should be used, and the surface of the right side of the abdomen gently palpated. It will then usually be found that the focus of greatest sensitiveness is on a line joining the umbilicus with the anterior superior spine of the ilium, and in the adult about two inches from the latter point; this corresponds with the position of the base of the vermiform appendix. In two cases of simple appendicitis which I recently observed, this proved a very definite and useful

symptom. A fulness may also be detected in a few cases, but at the onset a distinct tumour is rarely to be felt. The tendency of this simple form is to rapid resolution; within 24 hours an improvement in the symptoms is usually noticeable, and the patient is convalescent in a few days; in some cases, however, there are frequent recurrences, while it must constantly be borne in mind that even the most simple variety may at any moment merge into one of the graver forms.

The second variety is characterised by the formation of localised abscess, and where the symptoms of the simple appendicitis do not subside in the first day or two, but on the contrary gradually increase, the formation of pus may be expected, and if a tumour is present and the skin becomes œdematous over the iliac fossa, the diagnosis may be made with confidence. The ordinary iliac or lumbar abscess which may ensue as a result of appendicitis possesses no very special points of interest, and all are agreed that when of large size and very obviously fluctuating it should be opened and drained in the usual manner, without any attempt to extirpate the appendix. Should a faecal fistula ensue it will in all probability close after a time and recovery will be complete, or if a permanent fistula remain it may be dealt with more safely at a future date.

Where, instead of the moderate abscess occupying the iliac fossa or lumbar region, we have an intra-abdominal abscess of considerable size, the diagnosis may not be so easy, while the treatment must be prompt. I can best illustrate this variety by giving the outlines of two cases which have come under my own observation. The first was that of a young lady, aged eighteen, that I had an opportunity of seeing through the kindness of my colleague, Dr. Walter Smith, with Dr. Fitzmaurice, at Tralee. When I saw her, March 25, 1890, she had been suffering from fever and great pain in the right side of the abdomen for three weeks.

There was a tumour considerably larger than her head projecting very prominently directly forwards in the right side of the abdomen, and the intestines were all pushed to the left side, which was not much distended. The tumour was markedly tympanitic all over, and I was told that it had increased very rapidly within the previous few days. I made an incision into it and the swelling at once collapsed, giving exit to a quantity of abominably offensive gas, and but a few ounces of thick pus. The wall of the cavity was formed of intestines matted together by adhesions, and the abdominal parietes. It was deemed inadvisable to make any prolonged search for the appendix, and a large drainage tube was inserted. The improvement in the patient's condition was immediate, and the abscess contracted to a fistula, from which flatus and a small quantity of fæces escaped; this gradually contracted and finally completely closed, but not for over a year after the operation. She has recently been up in Dublin in perfect health.

The second case was a female, aged thirty-five, whom I saw in consultation with Dr. Flinn and Dr. Wallace Beatty, January 22, 1891; the appearances were almost identical with the last case, except that the tumour was larger, quite as large as a gravid uterus at full time; it projected directly forwards and overlapped the right thigh; it was tympanitic all over. An incision was made, and again a large quantity of fetid gas escaped and only a small quantity of pus; the cavity was washed out and a large drainage tube introduced, no search being made for the appendix. In this case a fistula also remained, but otherwise the patient rapidly regained health; it is, however, many months since I heard of her, so I am unable to say whether the fistula has yet finally closed.

When extravasation of fæces suddenly takes place from a perforated appendix unlimited by adhesive inflammation,

diffuse septic peritonitis is the necessary result. I am convinced that the majority of cases of so-called "idiopathic" purulent peritonitis in young people have their origin in perforation of the appendix. Such cases are to be diagnosticated by the much greater severity of the initial symptom, the frequent weak pulse, and the rapid onset of collapse and meteorism. Under these circumstances the sole chance of saving the patient's life lies in immediate laparotomy, and the surgeon who shirks this duty incurs a grave responsibility. I again crave your indulgence to introduce two illustrative cases, which, however, were attended with different results:—Miss C., aged sixteen, was seen with Dr. Wright, of Dalkey, and Dr. Walter Smith, November 4, 1891. Three days previously she had been seized with violent abdominal pain and vomiting, followed by collapse. When I saw her she complained of the greatest pain on the left side of the abdomen, but the symptoms being held to justify the diagnosis of peritonitis, an exploratory laparotomy was performed. The moment the peritoneum was opened gas escaped. On searching the left side, where pain was most complained of first, it was found that the pain was caused by a spreading peritonitis in that direction, but that as we passed to the right the evidences of inflammation were more marked, the injection of the surface of the intestine being more intense and flakes of lymph more numerous. On raising the cæcum thin purulent fluid escaped from the iliac fossa, and the vermiform appendix appeared intensely inflamed and with a ragged ulcer perforating it; there was no trace of limiting adhesions. The appendix was amputated, the cavity of the peritoneum cleansed as perfectly as possible, and the wound closed without drainage. The patient made a good recovery, and is now perfectly well.

In a second case which I saw with Dr. Tweedy and Dr. James Little—a male, aged nineteen, was operated on,

February 6, 1892, the third day of acute peritonitis. On opening the abdomen free gas escaped, the intestines were found violently inflamed and greatly dilated, so much so that they bulged into the wound, and it was only with the greatest difficulty that the appendix could be exposed. It was found perforated by an ulcer near its mesenteric attachment and amputated, the cavity of the peritoneum being cleansed as well as possible. As was to be expected from the condition in which we found the interior of the abdomen, no attempt to rally was made, and death ensued 28 hours afterwards.

In conclusion, I would venture to suggest that appendicitis should be treated on the following lines: Let every case be watched with the greatest care and visited at short intervals, remembering that while the majority will probably subside spontaneously, any one may suddenly develop into the more serious forms. To stop the vomiting by withholding food by the mouth is obviously sound practice, while anodynes in small quantities by limiting peristalsis may undoubtedly be useful, but given in large doses they may be a source of real danger, by masking the onset of grave symptoms which urgently demand operation. If at the end of 24, or at most 36, hours from the onset the symptoms are still *progressive*, the case ceases to be one of simple appendicitis, and the limits of purely medical treatment, I confidently believe, have been reached. There has within the last few years been accumulated abundant evidence showing that amputation of the appendix, if undertaken early, is almost uniformly successful, while the conditions found on opening the abdomen have often been such that recovery could scarcely have been hoped for by expectant treatment. By early operation the only hope is given to cases where diffuse peritonitis has commenced, and even in cases where abscess only is commencing, the patient is saved the risk of this bursting into

the peritoneum and so becoming generalised, while the tedious convalescence necessary when a large abscess cavity is drained is obviated. It may be urged that by resorting to laparotomy at this early stage cases will be operated on that would have recovered spontaneously. If due care is exercised this is not, I think, a contingency at all likely to arise, and we must remember that a carefully-done aseptic abdominal exploration, in skilled hands, is almost devoid of risk. To have explored a case which proved to be only simple appendicitis is, in my mind, a much less serious mistake than to allow a case to drift on into hopeless septic peritonitis for the want of timely interference. Speaking from my own limited experience of abdominal section generally, I have frequently had cause to regret that I had delayed operation too long, but never that I had opened an abdomen prematurely.

In cases of recurrent simple appendicitis, where the attacks are numerous, removal of the appendix may be called for. McBurney relates one in which he operated and removed an extensively diseased appendix from a patient who had twelve attacks in the previous twelve months. I recently saw two cases—one in a fourth attack, the other in a third—but as they both convalesced rapidly I did not advise operation in either.

DR. JAMES LITTLE spoke of the difficulty in making the diagnosis when there are only symptoms of general peritonitis, but no indication of localisation of the mischief in the cæcal region. Then there is the further difficulty of determining when there is little or no prospect of a cure taking place without operation. We all know that patients have recovered from repeated attacks of perityphlitis without operation, and the real difficulty is to know when there is little or no hope of recovery without surgical interference. Cases such as the two first narrated by Dr. Ball are plain enough: we have a localised abscess shut off from the general sac of the peritoneum, but such cases he thought rare, and the real difficulty arose in knowing when there is danger of general peritonitis; if

we wait until this has taken place, surgical treatment, however skilful, affords but a small chance of relief, and yet one hesitates to propose a laparotomy when the inflammation may remain limited to the post-cæcal region.

DR. HEUSTON called attention to the position of the abscess in relation to the peritoneal cavity as noted in Dr. Ball's paper. This is of importance, as it was so recently thought that perityphlitic abscesses were extra-peritoneal in their position, whilst in reality abscess was, in 99 out of 100 cases, within the peritoneal cavity, although probably localised by adhesive peritonitis in the immediate region. Among the cases he had seen, in no instance has the abscess been extra-peritoneal in its position; and with reference to the liability to diffuse suppurative inflammation, the speaker considered this is frequently to be accounted for by the position and anatomical relations of the appendix—viz., if this process be free and have a well-marked mesentery and projects over the brim of the true pelvis, there is a much greater liability for diffuse suppuration than if the process have little or no mesentery and be situated within the false pelvis posterior and internal to the cæcum. The importance of McBurney's spot in those cases depended, in his opinion, on the stage to which the case has advanced, as in the cases which have come under his observation, where diffuse suppurative inflammation existed, the seat of pain was nearly invariably to the left of the median line, so that he looked on this spot as being of value only in the early stage before diffuse inflammation has come on.

DR. H. C. TWEEDY drew attention to the great difficulty in making an accurate diagnosis in cases of perforation of the appendix followed by peritonitis. The situation of the pain does not always give early information. It may occur on the opposite side. It may occur in the epigastrium and elsewhere. The temperature is an equally fallacious guide, as in many cases the temperature may run an almost normal course, or else may be characterised by unaccountable variations. The pulse, as a matter of fact, gives a surer indication of the condition of the patient than any other sign. If the peritonitis be progressive the pulse becomes more frequent and thready, and this condition becomes more marked as the hours go on. It seems desirable to have the advantage of a surgeon's opinion, with a view possibly to an exploration. Public opinion is happily becoming more educated on this point, and more alive to the fact that the danger of opening the abdomen under proper

aseptic precautions has now been reduced to a minimum, and therefore in cases where the symptoms continue urgent after 36 hours (as suggested by Dr. Ball in his paper) the danger of delay should be clearly explained to the patient's friends and the necessity of an operation insisted on, which will in many cases be instrumental in saving life.

DR. WALTER G. SMITH thought that the important point for the physician was a careful balanced judgment upon the merits of each case rather than reliance upon definite rules as to time of interference. Omitting the cases where the accuracy of the diagnosis is confirmed by the abdominal section, three surprises may await the physician and surgeon—(a) The diagnosis may be wrong, yet the condition of things found may be remediable by operation. (b) Something unexpected may be found, irremediable by operation and fatal in result. (c) Nothing definitely wrong may be discovered, and yet the patient may recover, and an inexplicable cure has often resulted. In cases (a) and (c) the result is equally satisfactory to the patient; in (b) death is inevitable in any case. There is little danger now-a-days of premature operation, whereas it is certain that many patients have been lost through delay, ignorance, or timidity upon the part of the medical attendant.

DR. J. W. MOORE expressed his entire appreciation of the triumph of surgery in cases of deep-seated intestinal inflammation. At the same time he adduced two instances of such accidents in which patients, whose condition at one time was critical in the extreme, made good recoveries without any surgical interference whatever. Of these the first was a case of perityphlitis occurring in a young gentleman, in which the late Dr. Robert M'Donnell gave valuable surgical advice. Purulent diarrhoea suddenly occurred, and the patient made an uninterrupted recovery. The second case was that of a middle-aged woman, in whom a perinephric abscess finally discharged by the bowel—a communication having been established with the colon after some weeks of gravest illness. Large quantities of pus escaped by the bowel, while the urine also deposited pus in astonishing amount, owing, apparently, to pyelitis.

DR. R. A. HAYES related a case which illustrated one difficulty in diagnosis occurring in a boy, aged fourteen, who exhibited all the symptoms of acute typhlitis with general peritonitis, but who subsequently proved to be suffering from enteric fever, and finally recovered.

DR. PARSONS believed that, for practical purposes, there were

three varieties of typhlitis—(1) simple, (2) associated with formation of abscess, (3) associated with diffuse acute peritonitis. Ninety per cent. of all cases of typhlitis are stated to recover under purely medical treatment. There could be no doubt as to the necessity for surgical interference for the second class, and the great difficulty consists in making a diagnosis between class one and three, but a steady increase in the frequency of the pulse, combined with collapse, pointed strongly to perforation. For such cases operation was the only hope, and no time should be lost in opening the abdomen.

DR. H. T. BEWLEY believed that cases of simple perityphlitis very often lasted a week or more, and then gradually got well without any evidence of an abscess having formed, and in which no diffuse peritonitis occurred. He had himself treated five cases of perityphlitis, in which, after a week or ten days, the symptoms gradually subsided, and which ended in recovery. He objected to the rule, laid down especially by American surgeons, that if a case was not better at the end of 36 hours it should be operated on.

DR. BALL, in reply, thanked the physicians, especially his colleagues, for the facilities afforded him of early consultation in cases of peritonitis. He agreed with previous speakers in the great difficulty of diagnosing the source of early peritonitis, but thought the chief thing was to diagnose the fact of its presence in deciding on laparotomy. He also agreed with Dr. Parsons in thinking a rapidly-increasing pulse a far more reliable sign than high temperature alone. The limit of 36 hours did not mean that the patient was well by that time; but if the disease was markedly progressive at that time it might be held to indicate one of the more severe forms.

CASE OF INTRA-LARYNGEAL TUMOUR.

By ROBERT H. WOODS, M.B.;

Throat Surgeon to Richmond Hospital.

THE patient L. W., aged sixteen, was admitted to the Whitworth Hospital on October 30th, 1891, suffering from dyspnoea and aphonia. Two days later her breathing became so laboured that she was transferred to the Richmond Hospital with a view to tracheotomy should her condition get worse.

She stated that three months previously she noticed lumps in her neck which grew very rapidly in size, and that her breathing was first affected a fortnight before her admission to hospital.

Her aspect was strumous, the upper lip somewhat thickened and slightly prominent, with a heavy expression about the eyes. The lymphatic glands of the neck, especially those in front of the sternomastoid, were very much enlarged, there was also considerable infiltration of the tissues in front of the trachea.

On laryngoscopic examination the cause of obstruction was seen to be a greyish-red somewhat polypoid tumour in size about a small hazel nut, situated in the subchordal space, with its base attached to the anterior wall of the larynx immediately below the true vocal chords. It reached backwards, encroaching on more than half of the air space. Its pedicle was thick, and blocked the angular interval between the subchordal mucous membrane on either side. The tumour was seen to move slightly up and down with the current of air, but owing to the thickness of the pedicle, if indeed it can be said to have had any true pedicle at all, and the way in which it was sup-

ported on either side by the walls of the larynx, the range of movement was very limited. Added to the tumour there was some infiltration of the mucous membrane at the posterior part just below the arytenoids, so that the relatively small space left by the tumour was encroached on by this infiltration. The rest of the larynx was quite normal. I was enabled to demonstrate the growth to the members of the class round the bedside.

Of the nature of the growth it was not easy to be sure. There was no history of specific disease, and non-specific growths of the air-passage below the true vocal chords belong to the rarest affections. But, taken in conjunction with the glands in the neck and her general aspect, our idea was that the growth was an inflammatory neoplasm probably of strumous origin. She was accordingly put on large doses of the syrup of the iodide of iron.

On Sunday night, November 1st, two days after the patient had been transferred to my charge, her breathing became so bad that I was forced to do tracheotomy. After a somewhat tedious dissection I came on the trachea a good inch from the surface, opened it above the isthmus of the thyroid and introduced the tube. Owing to the depth at which the trachea was situated the ordinary shaped tube did not fit very perfectly, but after a search we found one that served the purpose very well. This afforded her great relief.

Five days afterwards her condition was so much improved that I took out the tube, holding it however in readiness to be introduced at a moment's notice should her condition require it. She improved rapidly, and twelve days after operation the tumour had diminished to about half its former size, the infiltration at the bases of the arytenoids was very much less, and on the twentieth day after operation she was well enough to be discharged.

It is now nearly four months since the operation, and there is not a trace of the tumour; her voice has completely returned, and she is in every respect well.

DR. N. FALKINER asked Dr. Woods if there was any discharge of pus or serum during or subsequent to the operation which would point to the tumour as being an inflammatory growth.

DR. HAYES congratulated Dr. Woods on the result of his case, and mentioned, as an interesting fact, that new growths of the larynx appear to be comparatively rare in Ireland. He considered that the patient's recovery was accelerated by the physiological rest afforded the larynx by the tracheotomy.

DR. WOODS, in reply, attributed the recovery partly to the physiological rest afforded to the larynx by operation and partly to the influence of iodide of iron. He quite agreed with Dr. Hayes as to the rarity of laryngeal tumours in this country, his experience going to show that they were confined for the most part to carcinoma and tertiary syphilis.

SECTION OF SURGERY.

PROSTATECTOMY.

By R. F. TOBIN, F.R.C.S.;

Surgeon to St. Vincent's Hospital.

[Read in the Section of Surgery, November 13, 1891.]

I SHALL make no apology for the brevity with which I am about to refer to the important subject which I have the honour of bringing before the Academy this evening. The presence of so many men of great ability and experience as I see here stimulates me, not to delay you with remarks of my own, but rather to roll in amongst you, if I may so put it, an apple of discussion. Situated as we are in Dublin, no one of us, however zealous he may be, can command a large number of cases of a given kind, and, therefore, it is only by utilising the experience of one another that we can advance to safe results. I propose to lay the subject before you, and to ask for your opinions under three heads—

1st. Suggestions as to treatment that may, in certain cases, delay or altogether obviate the necessity for operative interference in senile prostatic enlargement.

2nd. The indications for operation.

3rd. The method of operating.

Before proceeding to the little I have to say under these heads, permit me to call attention to a point in the pathology of the disease which seems to me to bear directly on treatment. In the various specimens of this disease which

I have examined, whether in the operating theatre, the *post mortem* room, or the museum, I have been struck by the irregularities of prostatic enlargements, and by the number of these enlargements that by their shape lend themselves to operative interference. Here on the table are a number of such specimens. Further, if you look into the history of these cases, you will find that growths of a kind that are easily removable are those that cause most obstruction to the out-flow of the urine. In our own museum here, the value of an excellent collection is greatly diminished by the fact that only to a few of them is the history of the case appended in the catalogue. One of these few clearly illustrates what I mean. It is specimen F. b. 228. It is described in the catalogue as follows:—"Enlargement of the third lobe of the prostate, the lateral remaining unaffected. It forms an oval tumour, about the size of a walnut, occupying the neck of the bladder, and projecting forward in a very marked manner. In this case there was no difficulty whatever in passing a catheter into the bladder, the tumour being movable, and yielding readily to the pressure of the instrument. Its mobility was, however, on the other hand, most disadvantageous to the patient, as the moment he attempted to make water it was thrown as a valve against the urethral orifice, so as to prevent the escape of a drop of urine."

The very last case I saw on the operation-table was instructive in the same direction, but to a greater degree. It was in the practice of my colleague, Mr. M'Ardle. In this instance the obstruction was such as to make the patient demand operation, yet the cause of the obstruction was found to consist in two small tumours, each about the size of a hazel-nut, which were easily removed. Nearly all authorities recognise the condition to which I am referring, and I dwell on it only because, while being so recognised, the lesson which it teaches seems to have been missed.

Sir H. Thompson says:—"Simple tumours, or outgrowths from the prostate, are, it cannot be doubted, present in a large majority of the cases of hypertrophied prostate."

Sir J. Paget states:—"Detached outlying masses of new substance, and like prostate gland in tissue, may sometimes be found," and he gives the following case:—"A man sixty-four years old, who for the last four years of his life was unable to pass his urine without the help of the catheter. He died with bronchitis, and a tumour measuring $2\frac{1}{2}$ inches by $1\frac{1}{2}$ inches was found lying loose in the bladder, only connected to it by a pedicle, moving on this like a hinge, and when pressed forward obstructing the orifice of the urethra. Now, in general aspect and microscopic structure, this tumour is so like an enlarged portion of the prostate gland that I know no character by which to distinguish them."

Velpeau, in referring to these enlargements, describes them as fibrous prostatic tumours which are developed in the direction of the vesical cavity, and which become absolutely pedunculated precisely as do the fibrous polypi of the uterus.

It would help greatly to elucidate this important part of the subject if, in any discussion that may take place, whoever have made *post mortems* in cases where there was complete retention from prostatic enlargement would tell us whether such local development of hypertrophy as I have referred to existed or not.

With this reference to the pathology of the disease I shall proceed to make a few remarks under the three heads already given, and if in doing so it appears to you that what I have to say is disconnected, I hope you will excuse me, as I mean to refer to nothing that has not come within my own experience.

First, as to suggestions other than operative, and other than the ordinary routine treatment with which we are all so familiar, and of which, no doubt, we all approve.

Under this head I have to make only the following remark ; but if it is true it is not unimportant :—I think that, although for people in a sound condition the upright position is the easiest in which to pass water, people afflicted with prostatic obstruction will be found to pass it more easily and more fully lying on one side, with the front of the body looking a little downwards. In this position the enlarged middle lobe and the pouch behind it are not favourably situated for valvular closure, as is the case when the patient is erect. Anyone who studies the specimens in our museum cannot fail to come to this conclusion, and anyone who recommends it to his patients will, I believe, find that in a majority of cases the practice verifies the theory. In one instance in which I made this suggestion lately, I was at first disappointed in the result. The patient said that he found the action in question as difficult when he was on his side as when he was erect. I thereupon took the trouble to inspect him during the performance. I found that his bed was next the wall, so he, in carrying out my instructions, turned not to the side he felt inclined, but to the only side he could, since, in order to do what I suggest conveniently, it is necessary to move to the edge of the bed. On his bed being reversed he found the greatest relief and benefit from the change. Owing to the extremely irregular way in which the prostate enlarges, it, of course, stands to reason that the position that suits one patient may not suit another. My suggestion, therefore, is that each patient should be encouraged to find out what position suits him best, and having done so, to empty himself, when occasion requires, by position rather than by muscular efforts which, when ineffectual, tend to aggravate the condition they are striving to overcome.

There is a second remark I am tempted to make under this head, although I have said I would make but one. In the treatment of the occasional attacks of cystitis that arise in

prostatic cases I have found bromide of potassium in ten-grain doses give very great relief by dulling the sensibility of the bladder.

The next question is—What are the indications for operation? I will summarise my conclusions in a few words. As long as a patient can carry on at all conveniently by artificial emptying of his bladder or otherwise, no operative procedure should be undertaken; but it should not be delayed where the obstruction is difficult to overcome, and where the general health is suffering in consequence. In cases where the obstruction to micturition is complete, or nearly so, and where, on examination by the finger in the rectum and the sound in the urethra, the gland is found not to be greatly enlarged, a lobe that is movable and, consequently, removable may be suspected, and an operation hopefully undertaken.

The next question is—What operation? I submit that all operations through the perinæum—even exploratory operations—being necessarily of the nature of blind surgery, are contrary to the spirit of to-day.

Here are two specimens of prostates that have been tun-nelled through—one successfully, the other not so. In the successful case the water would not pass through the tunnel without the use of an instrument. In the other, where the instrument passed, as may be seen, outside the prostate and entered the bladder above it, the patient lived only a few days. In my experience I have never come across a case successfully operated on through the perinæum, and I would like to know whether your experience in this matter agrees with mine.

I have said that even exploratory operations through the perinæum are blind. I happened, when in charge of the surgical division at Netley, to have a large number of bladder cases under my care just at the time when Sir H. Thompson

was writing up perinæal incision as a means of exploring and draining the bladder.

Moved by his impressive style, and by the ease and safety of the operation, I performed it in a large number of cases; but, though I found it useful for drainage, I found that, except in rare cases, the information it gave me as to the state of the bladder was small. You cannot get your finger sufficiently far into the bladder to feel round you, and the parts of the bladder that you can press against your finger by means of your other hand are seldom those that you want to examine. Later, when I came to examine prostatic cases in this fashion, I found myself in a still worse plight. The length of the urethra makes it difficult in such instances to get the point of the finger into the bladder at all, and when it is there it is powerless to perceive the condition of the parts immediately surrounding it. It is in one sense farthest from that to which it is nearest. It can be best likened to a man in a high collar trying to study the condition of his own neck-tie.

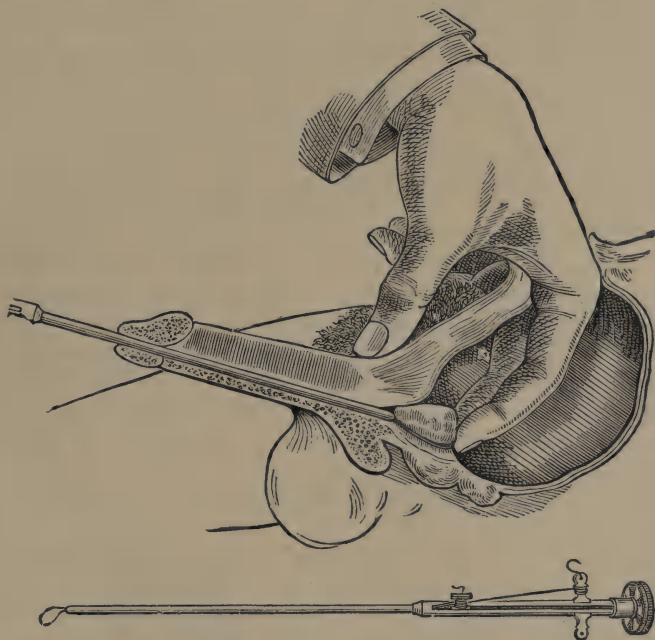
Amongst the many gains to be derived from the revival and development of suprapubic cystotomy, I imagine that those which will fall to the share of the diseased prostate will not be the least. We are all, I take it, aware of what has already been done in this direction, especially by Mr. M'Gill, whose untimely death has been such a loss to surgery. The initial steps of the various operations that have come to my notice are the same, and the chief difference consists in the means employed in dealing with the diseased gland. It has been cut away with knife and scissors, it has been pinched away with forceps, and it has been, if I may use the expression, strangled away by the wire of an appropriate *écraseur*.

What my views are on the method of operating I can best lay before you by reading a case which I have already published.

CASE.—J. K., aged sixty, was admitted 3rd May, 1890, into the St. Vincent's Hospital, Dublin, for retention of urine due to an enlarged prostate. For eleven years he had suffered more or less from this ailment, and had been treated for it both by me and my colleagues. On the last of these occasions, about four years ago, I made a digital examination of his bladder through a perinæal incision, but not being able to ascertain thereby anything more definite than what was already known, that there was an enlargement of the prostate, I used the incision merely to drain the bladder and cure thereby a cystitis that was then adding greatly to his sufferings. The history of the years that intervened between that operation and his admission on the present occasion is the ordinary one of a poor man who cannot make water without the use of an instrument. A rough catheter and a raw urethra sketch it sufficiently. When admitted he was emaciated and in a bad state of health, there was a tendency to the formation of bed sores on every prominence, the urine was fœtid, he had hourly calls to micturate and could do so only through an instrument, the passage or retention of which gave intense pain.

The following operation was performed on 9th May:—A suprapubic cystotomy having been done in the usual way, a digital exploration showed the middle and left lobes of the prostate to be much enlarged. The enlargement was a ridge, about an inch in height and an inch at its base, surrounding and overlapping two-thirds of the urethral orifice. Although the whole appeared to be one mass it was only so by contact, for the finger could be passed between the lobes. The instrument used to shave off this protuberance was an *écraseur* extemporised in this way. A wire doubled and bent to a suitable curve was passed through the urethra into the bladder. A silver catheter was then slipped up along it, but its point in place of being passed into the bladder was made to impinge against the urethral obstruction. Next the double wire lying in the bladder was opened out into a loop and slipped over the enlarged middle lobe and was kept embedded round its base by means of two fingers passed through the abdominal incision. While this wire was being made to cut its way through the part it encircled, the inserted fingers, helped by the point of the catheter, which was depressed to the spot it was desired to reach, regulated its direction. I was surprised how little resistance there was to its progress. As the lateral lobe was not of a shape to be encircled by the wire it was necessary to divide it into two by

nicking it deeply with a scissors. Each half was then dealt with as had been the middle lobe. The surface left was found to be smooth and to slope into the urethra. There was little or no bleeding. Copious irrigation with warm boracic lotion and a partial bringing together of the edges of the wound (the low vitality of the patient gave no prospect of immediate union) completed the operation.



Throughout its performance my colleagues Mr. M'Ardle and Mr. Patteson gave me, as they always do, most valuable assistance and suggestions. The chief points noted during the subsequent progress of the case were sloughing of the cellular tissue in the immediate neighbourhood of the abdominal incision; extension of the bed-sores noted as existing when admitted; uræmic delirium for ten days with a large amount of albumen in urine (about one-sixth). These complications notwithstanding, the patient made a good recovery. Most of his urine passed away through an india-rubber tube to a vessel alongside his bed. What urine overflowed was absorbed by large peat-moss cushions placed under the patient.

The house diaphoretic mixture containing liq. ammon. acet. pot. nit. and spirit. æther. nit., seemed to favourably influence the uræmic condition.

On the 22nd day after the operation the tube—a smaller one than originally inserted—was withdrawn from the abdominal wound, now nearly healed, and a catheter was passed by the urethra, and left in.

On the 16th June the patient was allowed up, and a few days later, about seven weeks after the operation, on being asked how he was, he answered, "I can make water as when I was a boy of fifteen." This statement must be taken rather as expressing his wonder at the change that had been effected in him than as an accurate statement of his condition. As a matter of fact, he could retain about half a pint of water, and pass it without pain or difficulty in the usual way. A week later he was discharged.

The parts removed are three masses, each about the size of a small walnut. Mr. Patteson, who has microscopically examined these, has found in them nothing but hypertrophied prostatic tissue. An *écraseur* which I have had made for use in future cases is here delineated. It requires no explanation, except that it is straight, so that its point may be accurately fixed against the obstruction to be removed.

The method of resecting the prostatic gland here set forth has, I think, the following advantages:—

1. As much of the gland as interferes with the escape of the urine is removed, and no more.
2. Such portion is removed in a satisfactory manner, for the wire cuts up to the point where the instrument has been stopped by the obstruction.
3. A smooth surface sloping into the urethra is left, instead of the more or less rough one that must result from taking away the gland piecemeal with a forceps.
4. There is very little hæmorrhage.

I do not know that in all cases a wire would cut its way through an enlargement as easily as it did in the instance now related; but this difficulty could be overcome by preparing a bed for it with the point of a knife or scissors. Only in the coverings of the gland need any resistance be expected, its friable substance is easy of division with any instrument.

Nov., 1890.—It is now six months since the performance of the operation. The outlet from the bladder continues quite free, and

the power of retention improves with the lapse of time. The general condition of the patient is also much improved.

To what is here set forth I have only to add my views as to the position in which the patient should be placed. When the pelvis is well raised the operation is, I believe, greatly facilitated. This can best be done by the employment of a suitable table. I send round a photograph of that used by Trendelenburg, of Bonn. When no such table is at hand, the patient can be held in a good position by an assistant, standing with his back to the light, taking the patient's legs over his shoulders, and then bending forwards till only the patient's head and shoulders remain resting on the table.

I shall not delay you with any further remarks. It is unnecessary to remind you that when operating on men advanced in life, and on parts undergoing pathological changes, we must not expect any very brilliant results. When dealing with the young we may talk of making them "well;" when treating the aged we must be content with making them "better."

MR. HAMILTON said he would be glad to know if the author practised distension of the rectum and bladder. He was of opinion that these proceedings are quite unnecessary. A surgeon with a schoolboy's knowledge of anatomy can enter the bladder with safety to the peritoneum, if he keeps close to the pubes, and uses his knife with care, and his index finger with resolution and intelligence. The bladder having been opened, the removal of calculi could be easily accomplished, as well as of growths projecting from the vesical wall—such as the middle lobe as we so frequently find it. But the difficulty is much increased when we come to deal with the symmetrical enlargement of the lateral lobes. In such conditions he thought the most efficient instrument would be Volkmann's spoon. The tissue of the prostate is friable, and easily broken down. This practice is open to two dangers—hæmorrhage and poisoning. The first is to be guarded against by not

scraping beyond the capsule so as to wound the veins of the prostatic plexus, so frequently varicose in this condition. The second can be obviated by careful antiseptic irrigation. Mr. Tobin had alluded to the condition of the lining membrane of the bladder in such cases. He (Mr. Hamilton) had great faith in the use of triticum repens, but the usual medicinal doses are simply inert. To be effectual it must be taken as an ordinary drink.

MR. WHEELER said that Mr. Tobin's discourse might be considered under two heads—cases in which operation could be performed to permanently relieve, and those in which suffering could be alleviated and life prolonged. Under the first class the obstructing growth of the prostate gland could be removed, in which surgical experience was limited. He had operated by supra-pubic incision to give such permanent relief, and to remove two growths from the lateral lobes of the prostate. This patient suffered from vesical hæmorrhage, constant desire to void urine, and very contracted bladder. A supra-pubic incision revealed the growths, and also papillomata studded all over the surface of the bladder. The prostatic growths he ligatured and nipped off with a curved scissors. The ligatures came off during the operation, no hæmorrhage followed. The papillomata he rubbed off with a sponge; the bladder was drained by a soft tube through the pubic opening, one end being received into a urinal. This patient made a good recovery. A period of nine months having elapsed, he advised the pubic wound to be closed, but the patient felt so comfortable he would not consent. He had performed operations by the supra-pubic method to alleviate suffering, and the trouble and irritation consequent upon enlarged prostate. In those unfit for further procedure he always used and preferred soft flexible tubes to silver instruments. Patients found them more comfortable. It was an advantage to steady the end of the tube in the bladder with some small weight; a shilling attached to the end of the tube answered well, and not only steadied it but sunk it to the lowest part of the bladder. Advanced lives were thus prolonged, and with comfort. It was by no means easy to clearly see the interior of the bladder by this operation. Artificial light was a great aid. His experience taught him that patients with the prostate enlarged passed water more effectually leaning on their hands and knees. In these cases he preferred draining the bladder above the pubes. The median operation did not afford so efficient a drain in cases of enlarged prostate, and after this operation the gland may get temporarily

swollen. He did not agree with Mr. Hamilton that there was not any danger in wounding the peritoneum in contracted bladders. The operator cannot be too careful.

Mr. M'ARDLE could not see the necessity for Mr. Wheeler's elaborate but unsurgical method of supra-pubic drainage while we had the simple but perfect tubes invented by Guinard for that purpose, nor could he agree with the opinion of that gentleman as to the thorough efficiency of supra-pubic drainage. There could be no doubt in the mind of any surgeon at all acquainted with this department of surgery that perineal drainage properly carried out is the most perfect. So valuable is it that Keyes, of New York, devised a special means of conducting the perineal tube through in supra-pubic operations of the class under discussion. Mr. M'ARDLE had yet to see an instance of supra-pubic drainage which was anything like complete in all the positions a patient may assume, while he found a properly applied perineal tube to effectually drain the bladder and allow the most thorough irrigation. Mr. Hamilton spoke of Petersen's bag as if it were one of the essentials of this operation, while as a matter of fact it is not only useless in prostatectomy, but positively harmful. Experience of its use shows that the prostatic portion of the bladder is the point raised by the rectal bag, and its elevation means a narrowing of the field of operation. The proper support is obtained by the index and middle finger of an assistant pressing on the prostate through the rectum. Mr. M'ARDLE could not agree with those who said that the number of cases demanding this operation were few. His impression was that, when the value of the operation described by Mr. Tobin became apparent, the cases demanding it would be found very numerous.

SOME CASES OF ENTEROTOMY AND COLO- TOMY.

By WILLIAM THOMSON, F.R.C.S.;

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THERE is no more remarkable development of modern surgery than the impetus that has been given to operative interference with the hollow viscera of the abdomen. The old terror, which, even in my student days, was held before us all, as to the almost certainly fatal results of interfering with the peritoneum, has vanished; but it was a very real terror with some even then. I remember the late Mr. Robert Adams once saying to me, after he had been present at an ovariotomy, "Thank God, I never had to do that." And his gratitude was a very fair reflex of the feelings of the senior surgeons of his day. It was the part of the younger men, with the audacity that belongs to youth, to show that the danger lay largely in bad methods, and that there were roads which, if followed boldly, but at the same time with care, led to safety. The success which has attended ovariotomy has been followed by the removal of diseased intestine, portions of the stomach, and of the bladder, and with an amount of success that is among the greatest wonders of modern surgery. It is true that the older writers have narrated cases of extraordinary character, but they are few and far between, and they were often the result of accident rather than of selection. But it is to our own time we must refer the deliberate operation upon the intestinal tract as a generally accepted surgical procedure. When Mr. Bryant did his first colotomy in 1859, it had been done in London only once in the

preceding ten years. The first case I saw was in 1876, and the second was one in which I was the operator. When I graduated I had never seen the operation done in any hospital in Dublin. I mention these points to show that operations upon the intestine were not always as common as they are now, and that within comparatively few years they were rarely practised.

Since I read a paper on lumbar colotomy in this Section two years ago, I have opened the ileum three times, and the colon five times, for the purpose of establishing an artificial anus. The two groups differ from each other in this—that the ileum cases were all done for acute obstruction, while the colon cases were, with one exception, done when there were no symptoms of acute obstruction.

CASE I.—A printer, aged forty; had been the subject of constipation for over a year. I was able to make out a tumour in the ascending colon, slightly movable, and somewhat doughy. In consultation the possibility of this being a faecal accumulation was accepted, and the man was treated accordingly; but no impression was made upon it. Later on he complained of irritation of the bladder, and an examination of the rectum revealed a tumour on the anterior wall in the situation of the prostate. In a few weeks the symptoms of complete obstruction suddenly supervened, and I was obliged to operate. A central incision showed the patient to be the subject of malignant disease. The omentum and the mesentery were studded with small, dark, shot-like nodules. The tumour in the ascending colon was about the size of my fist, and was close above the cæcum. Removal of it, with such an amount of disseminated disease, was impossible; I therefore determined at once to open the ileum. The artificial anus was fixed in the abdominal wound in the ordinary way. The patient never rallied completely, and died in four hours. The tumour in the rectum was found to be of the same character as the others, which were sarcomatous.

CASE II.—The next case was that of a woman aged fifty, who had been suddenly attacked by pain followed by collapse. She had been treated for obstruction by means of belladonna and purgatives

without benefit. Vomiting continued, the abdomen became distended and tympanitic, and when I saw her her condition was one of extreme peril; she was almost pulseless, and we decided to postpone operation for a short time until stimulating treatment could have some effect. As soon as possible afterwards I opened the abdomen; there was great difficulty in dealing with the distended intestine, and in endeavouring to trace the obstruction. I was able to feel a thickened, unusual mass near the sacrum, but I could not bring it up. The patient was almost moribund, and I had to hasten my work; I accordingly opened the ileum as before, and fixed the artificial anus in the wound. She had great relief from pain, rallied considerably, and there was a copious discharge from the bowel, and I hoped that all would go well; but she began to sink early in the following morning, and died in about twenty-six hours after the operation. At the *post-mortem* examination the obstruction was found. There was a mass of caseating glands over the lower lumbar vertebræ; some of these had quite broken down. The ileum, near its termination, was involved in this mass by old adhesions, and its lumen was reduced to the size of a goose-quill.

CASE III.—The next case was that of a young lady, aged twenty-one, whom I saw in consultation with Dr. G. H. Kidd and Dr. F. W. Kidd. She had been complaining for about a year, and had been treated for indigestion. When Dr. F. W. Kidd saw her she had considerable pain in the right iliac region, she was much constipated, had occasional vomiting, and had an elevated temperature. He discovered a tumour in the site to which she referred the pain. When I saw her the symptoms of obstruction had become urgent. The abdomen was distended, and the tumour could be felt somewhat above the cæcum. There was great tenderness everywhere, and her pain could be kept in subjection only by the use of morphia hypodermically. We agreed that an operation could not longer be delayed, and the next day I opened the abdomen—Messrs. Thornley Stoker and Corley, and Dr. F. W. Kidd assisting. The small intestines were greatly distended, and deeply congested. I at once passed my hand into the cavity, and examined the tumour; it involved the ascending colon, was firm and fixed, so that it was impossible to move it towards the surface for further examination. Believing it to be malignant, we agreed that there was no course left but to open the ileum. This was accordingly

done, and the bowel was fixed in the middle line. The patient rallied well, and was better next day. Her symptoms improved, but the discharge from the bowels never became solid. There was a constant escape of feculent fluid, which was so acrid that the skin of the abdomen became excoriated over a large extent, in spite of all our efforts to protect it. It would be tiresome to give all the details of this most painful case. Later on she was able to move to the country. In some months later the symptoms again became urgent, the tumour grew in size, ascites set in, and she finally died in January, 1890, six months after the operation.

My next group of cases consists of five, in which I performed anterior colotomy.

CASE IV.—A labourer, about fifty-five years of age, had complained of diarrhœa for some months, and had been treated by his doctor with various astringents. The number of motions daily were as many as twenty. He had pain, passed blood, and was emaciated. No examination of the bowel had ever been made. On passing my finger into the rectum I found a large ulcerated epithelioma about two inches from the orifice. It was quite fixed, all the surrounding parts were infiltrated, and the question of removing the mass was put aside. I accordingly determined to perform an anterior colotomy. A two-inch incision was made in the linea semilunaris, an inch above a line passing from the umbilicus to the anterior iliac spine. There was no difficulty in recognising the bowel, which was secured in the usual way, and opened at once. The patient did well, and lived for nine months, when he died from extension of the disease.

CASE V.—In July, 1889, a gentleman, aged forty-eight, consulted me for chronic diarrhœa. He stated that he had been suffering since the preceding January—that he had been treated for catarrh of the bowels and deranged liver. He had lost about one stone in weight; the motions were about twenty at night and six in the daytime. I asked if his rectum had ever been examined, and he said not. On passing my finger into the rectum I could just reach the lower margin of a tumour involving the whole wall. I could not detect any ulceration, and he said he had no pain, and he had not seen any blood. Dr. Ball met me in consultation at the patient's house next day, and after a full examination we agreed that

colotomy ought to be done. This I did, with the assistance of Dr. Thornley Stoker, in August. I determined to open the colon anteriorly, and at a much higher level than is usually selected, so as to get as far from the disease as possible. The point was somewhat above a line passing transversely through the umbilicus. I selected, as before, the *linea semilunaris*. Here I met with an unexpected difficulty. I found the colon easily enough, but it was for some time impossible to bring it out of the wound. Its ligament was extremely short, and when at last I was able to bring out a portion of the bowel and fasten it to the skin, there was so much tension that I feared the stitches would give way prematurely. The bowel was opened at once, but there was no discharge of *fæces* until next day. The patient was up in about sixteen days, and was able to travel to London on business on the thirtieth day after the operation. His subsequent history may be told in a few words. For nearly two years he was able to discharge very onerous public duties; the bowel acted perfectly about once a day. In July of this year he had ischio-rectal abscess, and there was evidence of extension of the tumour downwards. The liver became involved by secondary deposit; ascites followed, and the patient died just two years from the date of the operation.

CASE VI.—An old soldier, aged sixty-three. He had been suffering from intermittent attacks of constipation for two years, and had been treated from time to time in the Whitworth Hospital. There was no trace of disease in the rectum. On his last visit (July, 1890) for treatment he suddenly became very ill. The intestines were much distended, and there was frequent vomiting. He was removed to the surgical hospital, where I saw him. He was much collapsed. An examination of the abdomen showed great distension of the transverse colon, as far as the splenic flexure. I determined upon an exploration, and opened the abdomen high up on the left side, in the *linea semilunaris*. The small intestines gave great trouble, and it took some time to find the colon, which was at last recognised by the fingers grasping a small scybalous mass. Passing up to the splenic flexure, I found a mass of *fæces* there. There was obviously some narrowing at this part, but I was able to press the mass onwards. The question of opening the transverse colon was discussed, but the man was almost pulseless, and we felt that his life would be seriously imperilled by making a second incision. As the *fæcal* lodgment could be moved onwards

I believed that the way could be kept clear subsequently by the use of medicines and enemata through the opening in the colon. I then fixed the gut, and opened it. There was a free escape of contents. Next day the distension had disappeared, and the vomiting had ceased. The patient made an uninterrupted recovery, and the bowels acted with fair regularity. He died six months later of asthenia, but was not, I believe, troubled by any obstruction.

CASE VII.—A man, aged forty-two, sent to me by Dr. J. M. Prior Kennedy, of Tullamore, in June, 1890. For nine months he had suffered from alternate constipation and diarrhoea. At times he suffered much pain and passed blood. The cause of his troubles was recognised, and he was sent to Dublin. He stated that he had lost more than a stone in weight. The diarrhoea was very distressing, as he required to go to the closet sometimes 30 times in the 24 hours. An examination of the rectum revealed a hard, rugged, ulcerated stricture which would not allow the finger to pass through. There was extensive infiltration, and the diseased structures were quite fixed to the deeper parts. There was no course open but the performance of colotomy. For the first time I did it in two stages. The loop of bowel was drawn out and stitched through the serous and muscular coats to the skin. The dressing consisted of iodoform ointment applied under protective, so as to avoid the trouble of adhesion of dressings to the wound. On the fifth day I opened the bowel and clipped away the lips—a perfectly painless process. The patient left bed on the eighth day. The diarrhoea was quite stopped, the bowel discharged itself regularly, and the patient was able to leave hospital in three weeks. I have since heard that he suffered once from prolapse, which was reduced by Dr. Moorhead, of Tullamore, with some difficulty.

CASE VIII.—A woman, aged fifty, admitted August, 1890. Two years ago she had what appears to have been an ischio-rectal abscess, for which she was treated in a hospital. She said she received injections, and that the fluid returned through the vagina. Her condition was a very miserable one. The whole of the buttocks were riddled with openings. The rectum was occupied by a malignant mass, and the finger could be passed through the vagina into the bowel. She was disgusted with her own condition. I explained to her what could be done, but that her disease could not be cured; and as she was most anxious for some relief I

resolved to open the colon in front. This time I went a step further. The gut was found at once, and drawn out. I then passed a thick pin through the mesentery, and allowed the end of the instrument to rest upon the abdominal wall, transversely to the wound. Three or four light sutures fixing the bowel in position in the wound completed the operation, which lasted little more than a quarter of an hour. The bowel was opened three days later, and the patient was soon discharged.

There are a few points which these cases bring into prominence, to which I will allude as briefly as possible. It will be noted that, in the first group of cases of operation upon the ileum, I lost two out of three; and that in the second group in which the colon was opened all survived. There does not appear to be any special reason why the opening of the ileum should be more fatal than the opening of the colon, if the conditions are the same. Yet enterotomy, as it is called as distinguished from colotomy, is undoubtedly a very fatal proceeding. Treves collects 61 cases of non-malignant obstruction, of which 41 died within 10 days, and 24 within 48 hours. In 48 cases of enterotomy in malignant disease, 42 died—28 within 48 hours, and the rest within 10 days. The reason of this great mortality lies in this—that enterotomy is hardly ever done as a deliberate operation, but as a last chance in a case of obstruction in which that obstruction cannot otherwise be relieved. Then the case is usually an acute one; the intestines have become distended and deeply congested, and there is vomiting—in other words, we add to an already established serious shock the shock of an operation, which involves a search for the obstruction, and finally the opening of the bowel. In two of my cases the patients were already in a state of collapse when I operated. The same condition almost of necessity exists in all such cases, and under this accumulation of adverse circumstances, we have rather to congratulate ourselves when we save any.

Now, on the other hand, colotomy is usually done at a

selected time. There is not any serious obstruction, and all the conditions of time and the preparation of the patient are favourable.

All these cases of colotomy were anterior. On this question of anterior or lumbar colotomy very much has been written, and probably the result is that there will always remain upholders of each as the preferable method. My own experience is, that the anterior operation is in most cases more easy to perform, and that the position of the opening is more convenient for the patient than in the lumbar region. But it does not always happen, as some seem to think, that we make an incision through the peritoneum, and find the colon quietly waiting to be caught. Very often it is a bit of small intestine that presents, and we must search, and search diligently, before we find what we want. Mr. Allingham and Mr. Cripps, who have done the operation frequently, state that the colon is to be found at once only in one-half or one-third of the cases. In this respect the lumbar operation has the advantage, because the bowel is in more fixed relation to the primary incision. But I agree with Mr. Bryant and Dr. Ball, that in a case of much distension the lumbar operation is better, because we have not to dread the extrusion of small intestine should we fail to hit upon the colon at once.

The chief claim, however, of the anterior operation must be based on the fact that it is an abdominal section which gives us at once great possibilities. Sometimes it is not easy to localise a cancerous growth—such a thing as opening the colon in the lumbar region on the wrong side of the obstruction has happened before now. The anterior opening gives us all facilities in our search; we can enlarge it, we can examine the whole course of the colon—we may even excise a stricture. We may divide the gut completely, invert the edges of the lower part, and thus shut off the rectum

permanently from any chances of the irritation of the bowel contents. Now, only one of these advantages belongs to the lumbar operation, and for these reasons I believe that the anterior will gradually become the far more usual one.

In my last two cases I adopted the plan of dividing the procedure into two stages, and where there is time this is a great advantage. The parts are completely adherent before the bowel is opened, and there is then no fear of the passage of fæces into the abdomen. In the last case I used the pin for transfixion of the mesentery, thus, as it were, hanging the intestine in a loop over the steel bridge which passed from side to side of the wound. If the ligament is long enough to allow the bowel to be drawn out freely, this plan is most simple and satisfactory. The lips of the small wound lie close about the gut, adhesions take place in a very few hours, and there is really very little after-trouble. Everything is secure in a few days, and all that remains to be done is to open the bowel and pare away the edges—a proceeding which is curiously quite painless. But it has the further advantage that the pin so shapes the bowel as the adhesions become firm, that a very good “spur” is produced, and there is less likelihood of fæcal matter travelling down to the rectum. I may say, however, that, except in one case, there was no trouble from this, and that in the others the colon discharged all its contents through the artificial opening.

With regard to the position of the incision, I have made it at some point above a line passing from the umbilicus to the anterior superior spine of the ilium, and always in the *linea semilunaris*, as recommended by Dr. Ball, because the parts are thin, there is little or no hæmorrhage, and the rectus muscle is not interfered with. In the last case no vessel was ligatured.

As to the length of the incision, the smaller it is the better. I begin with two inches, and extend the opening if necessary

If the abdominal wall be fat the wound must be made larger, to allow of manipulation, unless the colon happens to be at once exposed; but in the majority of cases the wound was of small extent, and I certainly do not agree in the advice to make the wound four inches in length at the outset.

On the question as to when we ought to do colotomy for cancer some difference of opinion exists. Ought we to operate at once when the disease is discovered, although the symptoms may not be urgent, or must we wait until we are forced to do so? Malignant disease of the rectum is very insidious, and has usually been progressing slowly before the surgeon is consulted; but the facts that the necessary examination is made, and that the disease is discovered, show that there are already special symptoms present to indicate it—constipation, diarrhoea, wasting, pain, or bloody stools. The growth has reached a stage in its development when it manifests its presence, and has passed out of the period of slow and undemonstrative progress. I have myself no doubt that if any of the symptoms mentioned has become marked, and that the disease is recognised—always assuming that it cannot be directly removed—colotomy ought to be performed without much delay. It is reasonable to suppose that in a disease such as this the passage of fæces over it does not act as an emollient, and that the frequent action of the sphincters does not tend to soothe it.

One of the most frequent causes of epithelioma is continued irritation. We recognise that fact in all our practice, and we try to remove it. Rest is an important therapeutic agent elsewhere; why should it fail to be beneficial in the rectum? Therefore, I think that those who would wish to postpone the day of operation, perhaps deterred by the risk, are hardly giving effect to the principle which underlies their practice on other regions. At the present moment the anterior operation shows a mortality of 53 per cent., and the lumbar

of 31 per cent.—both sufficiently high to leave much room for improvement. But I think that if we knew all particulars of the cases we should find many in which operation came in as a last resource—when there was the shock of acute obstruction already present, as in my own enterotomies, and when everything favoured failure rather than success. Early operation can be done under the best circumstances. The advance of the disease is no longer helped by direct and increasing irritation, the comfort of the patient, as compared with his frequently troublesome diarrhoea or temporary obstruction, is secured as far as it is possible, and life is prolonged. For these reasons I believe we shall do best if we decide for action and not inaction.

MR. THORNLEY STOKER expressed himself strongly in favour of the anterior operation, which for nearly every reason he conceived to be the logical one, and with due precaution, and in spite of statistics, not more immediately dangerous. He also argued in favour of the performance of colotomy in cases of cancer of the rectum as early as possible, and that the procedure should not be delayed until malignant obstruction had taken place. He stated that while it was desirable to draw the bowel thoroughly into the wound and secure it there, by the pin passed through the mesentery, as practised by Mr. Thomson, or otherwise, he held this point as one of minor importance. It is sometimes difficult to perform this manœuvre owing to a short or fat mesentery, and it really does not matter much, as the distal lumen of the bowel always contracts, and it is found that even in cases where fluid can be injected from the rectum through the artificial anus, faeces do not pass in the opposite direction. Mr. Thornley Stoker also mentioned that he had of late adopted the practice in laparo-colotomy of securing the bowel in such a position that its distal portion is secured at the upper end of the abdominal wound and its proximal part at the lower end of the wound. When this can be done without twisting the gut—and it can generally be so done where the convoluted sigmoid flexure is the seat of operation—the effect is to make a sort of *cul-de-sac* on the proximal side of the anus in which faeces become stored and

are retained by gravitation. The fæces are then more likely to adopt the method of a periodic overflow and to avoid the more constant discharge which is often so troublesome, and which is more likely to occur when from the superior position of the proximal part of the bowel gravitation helps the continual discharge. This point he considered a novel one, and by attention to it he had added materially to the comfort of those patients in whom he had been enabled to practise it. He stated that this practice should only be pursued where it could be carried out by placing the bowel in the requisite position without any kink or twist.

MR. MYLES congratulated Mr. Thomson on the success of his cases, and commenting on them expressed some regret that Mr. Thomson had not said anything of the operation as an antecedent to the operation of rectal excision. Mr. Myles then mentioned a very successful case in which he had assisted Mr. Dwyer at Jervis-street Hospital to remove the rectum subsequently to a colotomy. Referring to Mr. Thornley Stoker's suggestion to turn the gut upside down so as to have a pouch below the aperture, he stated, in every case where the mesentery was of normal length, that part of the colon which lay between the descending colon and the new aperture descended into the pelvis so as to make a fairly effective pouch. Mr. Myles commended the use of the pin, asserting that no other method yet devised was as efficient in producing a valve so as to completely occlude the lower aperture of the bowel and prevent irritation of the cancerous sore in the rectum.

The PRESIDENT said he had performed colotomy in 15 cases by the lumbar method. There was no special difficulty in finding the bowel or trouble from the peritoneum. He had two deaths. Several of the cases lived a year. One case was at work as Corporation labourer at end of two years. There was no excoriation of skin. Early operation was important, and early examination of bowel with finger most important. In all cases of rectal trouble the rectum should be explored with the finger. He operated on one case which had been treated by a physician as one of dysentery before she came to the City of Dublin Hospital. Early colotomy stays the progress of the disease.

MR. WHEELER had performed both anterior and lumbar colotomy. He gave the preference to the latter.

MR. THOMSON, in reply, said he agreed with Mr. Thornley Stoker that the pin was not available in all cases. He had stated this in his paper. If the mesentery were too short to allow the

bowel to be brought outside the wound, the pin ought not to be used; but where a suitable case presented, the pin was undoubtedly most simple and convenient. He thought the four sutures which he used in addition were not necessary; but this was the first time he had used the pin, and he confessed that he was a little timid, and he wished to make assurance doubly sure. He had not dealt with the question of excision of the rectum in relation to colotomy, because that was a subject somewhat foreign to his paper, but it might be discussed on a future occasion. The President and Mr. Wheeler were in favour of the lumbar operation. He (Mr. Thomson) had admitted that there were some cases which were suitable for that operation; but he still held that the balance of advantage lay with the anterior method. It was certainly more convenient for the patient, who had to attend to himself, to have the opening in front, where it could be seen, and where any possible changes could be observed.

NEPHRO-LITHOTOMY.

By T. MYLES, M.D., F.R.C.S. ;

Professor of Pathology, Royal College of Surgeons ;
Surgeon to the Richmond Hospital.

[Read in the Section of Surgery, December 11, 1891.]

ALTHOUGH the operation for the removal of stone in the kidney has now been performed fairly frequently, nevertheless, as the subject is one of extreme interest and importance, I trust I may be forgiven for bringing before you a short notice of a successful case that occurred in my practice, not with the hope of being able to add anything to your general fund of information, but rather with the anticipation that the discussion—which I trust will follow the paper—may enable us to obtain the opinions and practice of men whose experience in this branch of surgery is greater than mine.

My case is that of a young man, who was brought to me by my friend, Mr. Louis A. Byrne, now Surgeon to Jervis-street Hospital, and whose history and symptoms suggested the possibility of his having a stone in the kidney. The symptoms were by no means clear or typical. He complained of dull, aching pain in the left loin, not very severe, and not paroxysmal; the pain was increased by effort, or coughing, and with this he had intense tenderness on pressure. He had no attacks of renal colic, nor any increased frequency of micturition, nor had he ever passed any calculous fragments with his urine. His urine was practically normal, acid in reaction, contained neither pus nor blood, and was normal in specific gravity and quantity. The young man himself—twenty years of age—looked in perfect health, having a good complexion, and being free from any emaciation or cachexia.

I had him admitted to Jervis-street Hospital, to which institution I had then the honour to belong, and he was seen several times by my colleagues in consultation.

As the symptoms were not conclusive, nor his distress urgent, I tried the usual palliative treatment for some time—hot baths, alkaline diuretics, opium occasionally, and hot stupes over loin.

His condition was not in any way ameliorated by this treatment, and he still complained of the same dull pain that he had previously suffered from. One day the sister in charge of the ward told me that she thought the lad suffered much more pain than he acknowledged, as she noticed him frequently when sitting at his meals clutch at the edge of the table, hold his breath, and look deadily pale and sick, as if suffering great agony.

On pressing him more closely he confessed that during the last few days he had suffered from these attacks, but he had not mentioned them, as he thought it might be the medicine he was getting that was expelling the stone, and that his sufferings were, therefore, merely the prelude to his complete recovery.

The diagnosis being now more satisfactorily established, I at once suggested an operation, to which he willingly consented.

I need not describe the now familiar details of lumbar nephrotomy, but I will merely state that after the usual incision had been made parallel to the lower border of the last rib, and one inch below it, and the perinephric fat exposed, on pushing my finger into the wound, and passing over the anterior surface of the kidney, which was apparently perfectly normal, I found a large, hard, tightly-fixed lump, which at first, from its position, I believed to be a bony tumour growing from the front and sides of the spinal column. On more careful manipulation, however, I soon had

the gratification of feeling that it was continuous with the kidney, and was in reality a large calculus imbedded in the pelvis of the ureter, which it carried somewhat inwards, and sending short processes outwards into the hilum of the kidney.

The difficulty now was how to remove it. On the one hand, it was obviously too large to be removed by cutting through the kidney periphery, and any attempt to remove it by cutting on the face of the pelvis was bound to give rise to dangerous hæmorrhage from the distended renal veins, which could be seen stretched right in front of the stone.

Acting on the suggestion of one of my colleagues, I determined to attack it from behind, and with that object I proceeded to gently raise the kidney off the quadratus lumborum muscle, and draw it forwards in the wound, so as to leave room for my fingers to touch its posterior and inner surface. I could now feel the calculus very clearly, and I ascertained that at one part the investing pelvis was extremely thin. Through that thinned portion I now scratched cautiously with a director, and made a comparatively small opening; a little gentle fingering now enabled me to revolve the stone upon its axis, and coax it out of the kidney. Through the wound in the pelvis I now explored the rest of the kidney thoroughly for another stone, but did not find any.

The wound was now well douched, all bleeding vessels clamped and ligatured, and the edges brought together with stout silk sutures; a large drainage tube was passed as far as the kidney, and fixed by a suture to the skin.

I will not weary you with a dry detail of his subsequent progress from day to day, but will merely state that, with one exception, the boy made an uninterrupted recovery, and I had the honour to show him last year at a meeting of the Surgical Section, perfectly well and able to follow his usual avocation.

The exception to which I referred was the fact that for

four days after the operation he had profuse hæmaturia—so much so, indeed, as at one time to cause me considerable anxiety. He was put on 10 minim doses of hazeline, which acted like a charm in arresting the bleeding; from that on his recovery was rapid.

This case of itself is not of any special interest, except from the fact that the symptoms were obscure at first, and that the stone was very large, weighing 282 grains, being, I think, the largest ever removed unbroken by any surgeon in Ireland, and, as anyone can see who examines it, it is almost a perfect mould of the pelvis and infundibula.

Although there is nothing very startling in the case, nevertheless I bring it forward in the hope that it may act as text upon which more skilful preachers may lay down the law, and some clear and definite decision may be arrived at upon some of the many interesting and as yet doubtful details of the operation.

1st. Having diagnosed the existence of a stone in the kidney, how should it be removed?—by the simple lumbar incision, or by the combined method of Thornton?

This latter method, which has been so strongly advocated by its originator, consists in opening the abdomen in the linea semi-lunaris, placing the hand in the cavity, and feeling the kidney for the stone, and having found it, the kidney is steadied with the hand in the abdomen, while the surgeon, turning the patient slightly on his side, cuts boldly down to the kidney from the loin by a short incision; the stone is then cut out and removed by the lumbar wound.

This seems at first rather a startling addition to lumbar nephrotomy, inasmuch as it superadds an abdominal exploration to the main operation; nevertheless, its author asserts that it has undoubted advantages, which he briefly specifies as follows:—

1st. Certainty that the patient has two kidneys.

2nd. There is less danger of overlooking the stone.

3rd. There is no danger of cutting into the wrong kidney—an accident of not uncommon occurrence.

4th. There is no danger of wounding either the colon or the peritoneum.

5th. As the wound is small there is no danger of a loin hernia, and less danger of urinary extravasation or fouling of the wound.

6th. The condition of the ureter and the other kidney can be ascertained.

7th. The kidney can be thoroughly steadied during the subsequent steps.

On the other hand, Mr. Thornton says the only objection to the method is the addition of another wound, which, he says, is of very little importance in the hands of a cleanly surgeon.

Although considerable difference of opinion may exist as to the value of some of these so-called advantages, there can be no doubt whatever that two of them are of great importance—namely, the assurance that you are cutting on the pathological kidney and not on the healthy one, and the benefit to be derived from having the kidney thoroughly fixed.

It may seem absurd at first sight to contemplate the possibility of cutting into the wrong kidney, but every practical man knows well that it may occur to the most careful surgeon, as it has occurred already to some of the most distinguished.

A patient with a stone in the kidney may refer the pain to the healthy side, and complain of no pain in the diseased side; or more frequently he complains of pain in both sides, and it becomes a matter of the most extreme difficulty to make sure whether there is not a stone in both kidneys, or if in one only, in which one?

In such a case Mr. Thornton says the abdominal section converts doubt into certainty, and by rendering a possible second operation unnecessary, adds greatly to the chance of a safe issue from the patient's danger. Now is this true? If it be absolutely true, I think no one would question the propriety of the method in all cases where there was any reasonable doubt as to the exact seat of the stone. But is it really absolutely true that an abdominal section enables a surgeon to discover the presence of a stone in the kidney?

I would, with great deference to such distinguished authority, venture to suggest that even the most careful and accomplished surgeon may fail to recognise a stone in the kidney by exploration with the hand in the abdomen; and I would go even further, and point out that it has occurred that, even after a kidney has been removed and laid on the table, the most careful palpation has failed to discover a stone actually in the organ.

If this be possible, even with a removed kidney, how much more probable does such a failure become when we endeavour to explore the kidney covered in front by colon and tightly stretched peritoneum, and with its hinder border and inner surface entirely out of our reach. Through such an abdominal wound no surgeon would venture to needle the kidney; and yet how often has the needle disclosed a stone unrecognised by the most careful palpation. Again, if the kidney is seriously diseased, and if its sinus is dilated by abscess formation, its coverings thickened by an inflammatory process, and the viscera matted together in front of it by localised peritonitis, how easily may a stone escape detection by palpation through an abdominal wound.

Further, we are all aware that small stones usually lie concealed in the calyces of the ureter deep in the kidney sinus, and that detection of such stones is frequently possible only by sounding through an incision in the lower border of

the pelvis, or through the kidney substance. How can we use this important aid through an abdominal incision?

I think I have said enough at least to show that Mr. Thornton's method, so far as its being an aid to certainty of diagnosis is concerned, is not altogether unassailable.

The other asserted advantage which I have, merely for purposes of convenience, placed second to the question of diagnosis is the claim that the kidney can be fixed by the hand in the abdomen, and the necessary manipulations for the removal of the stone be thereby more rapidly and successfully performed.

No one, I think, will question the statement that the great difficulty in lumbar operations arises from the fact that the kidney has an unhappy tendency to glide away from the fingers when the patient is lying on the opposite side, and this combined with the occasionally very narrow ilio-costal space renders manipulation through the wound difficult and tedious. Anything, therefore, which steadies the kidney must be beneficial, and the only point at issue really is, can this steadiness be obtained in no other way except by a *sectio abdominis*?

I think it can, and the alternative method is that mentioned by Mr. Thornton himself, of placing the patient almost flat on his back, with the wounded side overhanging a pretty high table, while an assistant applies to the front and opposite side of the abdominal wall steady and continuous pressure. By such a method the kidney can be fixed, and when we remember that in any case the stone must be removed by the loin incision, the adoption of such a method, if proved feasible, would render the abdominal wound unnecessary. Further, the so-called fixing of the kidney by pressure from within is not free from danger: it can only be obtained by traction on the fragile renal vein, and I need

hardly delay to point out the possible catastrophe that might result from such traction.

I have spent some time in considering these two reputed advantages of Mr. Thornton's method, because they are, to my mind, the only ones of real importance; the others are hardly worthy of notice.

His claim that his method prevents a patient with only one kidney being operated on, is simply puerile. It may prevent him from having his kidney cut open, but it does not hinder the abdominal section, and simple as that operation is, a patient with only one kidney runs a poor chance of surviving it. Again, I venture to state that the combined experience of this Academy would fail to register more than half a dozen cases out of the thousands of bodies examined in the dissecting and *post-mortem* rooms, in which only one kidney was found. So also the dangers of subsequent lumbar hernia, and of wound of the colon in the loin operation, are greatly exaggerated.

I think I am justified, therefore, in stating that I have shown that the claims which Mr. Thornton makes for his combined operation are based upon arguments that are at least open to question, and that the added dangers of the extra incision more than compensate for any possible advantages it may possess.

Before concluding, I would like to obtain the opinion of the Section on some details of the lumbar operation:—

1st. Is Mr. Jordan Lloyd's method of sounding each calyx by a child's sound inserted through the pelvis of the ureter feasible? At the very least, I should say it must be extremely difficult in all cases where the ilio-costal space is narrow, as owing to the depth of the kidney the movements of the sound must necessarily be very limited, and even its mere introduction cannot be easy.

2nd. The stone having been found, how should it be

removed? By section through the kidney substance, or by cutting into the ureter? Against the former is the danger of hæmorrhage and additional shock; against the latter the possibility of a permanent fistula.

I think a great deal of confusion has arisen in connection with this particular matter, by the very loose and unscientific way in which anatomical names are used. For instance, one writer says—"I cut into the pelvis of the kidney from the front." Now he could not do this, because the kidney has no pelvis, and if his statement means that he cut into the pelvis of the ureter from the front, I say that no surgeon in his sober senses would do it when he remembered that the renal veins and artery completely conceal nine-tenths of the pelvis, and that any attempt to do it would be simply the recklessness of anatomical ignorance.

What then do such statements mean? The answer is simply—nothing; the operator cut something, and, let us hope, found the calculus, but what he cut we do not know, and probably never shall. I may add, that even such an accomplished surgeon and anatomist as Mr. Treves is guilty of this unpardonable mistake, as in his recently published work on operative surgery, having first carefully described the relations of the pelvis, calyces, &c., he subsequently lapses into the error of speaking one moment of the pelvis of the ureter and in the next of the pelvis of the kidney. Such a lack of scientific accuracy can lead to nothing but confusion. No one knows what is meant, and the value of all descriptions of such operations is simply *nil*.

I will now conclude by thanking you and the members of the Section for the kind attention you have given; and as the object of my paper was to obtain information rather than to give it, I trust that the more experienced nephrotomists will give us their opinions on what I venture to call points of importance in connection with nephro-lithotomy.

MR. WHEELER said that he had performed nephrectomy on several occasions, but the communication only invited remarks upon nephro-lithotomy. He had shown calculi removed from the kidney of different individuals at this Society. He did not think that the abdomino-lumbar operation was necessary should the operator be quite certain that only a calculus existed, and that further operative measures would be unnecessary. He was of opinion that it did afford some help in diagnosis; but the combined operation had other advantages. In one of his cases there was much difficulty in detecting the stone—a small one—and he had to puncture the kidney seven or eight times before the needle impinged upon it. He then incised the kidney with a narrow-bladed knife and dilated with his finger. There was no hæmorrhage. The wound healed kindly. A tube was introduced as far as the site of the wound in the kidney. He had not found it necessary to sound the kidney with a staff. The so-called pelvic incisions should be made with caution. In one of his cases the fat was very abundant; he applied a ligature and cut it off. In cases of abscess he introduced the tube into the kidney.

The PRESIDENT, with reference to the difficulty of detecting the stone in the kidney by manipulation, mentioned a case in which he operated by the abdominal incision, and though he expected stone, it was not discovered until the kidney was removed and a section made of the viscus. A large stone was discovered. There was a large abscess. No renal artery could be found. No urine was secreted from the time of operation, and the patient died the night of operation. He made an examination of the remaining kidney, *post mortem*, and found a large stone in it also. This case showed the difficulty of detecting stone before operation, and of knowing the state of the other kidney.

BALLOONING OF THE RECTUM.

By E. HAMILTON, M.D., F.R.C.S.;

Surgeon to Steevens' Hospital; Professor of Surgery, Royal College of Surgeons.

[Read in the Section of Surgery, January 29, 1892.]

IN the *Lancet*, 5th January, 1889, Mr. Thomas Bryant has directed attention to a peculiar condition of the lower bowel. He has given it the very appropriate designation "Ballooning of the Rectum," and describes it in the following terms:—"The rectum in its normal condition is a collapsible tube, and when a surgeon introduces his finger through the sphincter into this tube in the healthy state he feels the walls of the bowel in contact, and these he has to separate to complete his examination. When a stricture of the rectum exists this condition does not always hold, and the surgeon will often find, when his finger is passed the sphincter, that he has entered a cavity the walls of which are expanded or "ballooned." In this cavity the surgeon will be able to move his finger freely, and the walls of the cavity will only be felt when searched for. The extent of ballooning will vary in every case. When this condition of bowel is met with the surgeon will be justified in more than suspecting the presence of stricture. For I have never found this ballooning of the rectum to be present *under other conditions* than stricture. In cases of obstruction complicated with symptoms which suggest the possibility of a stricture being their cause, the ballooning of the bowel becomes therefore a symptom of some importance, and is one which should materially help towards the confirmation of the diagnosis of stricture. I believe this condition of the bowel to be due

primarily to atrophy of the muscular coats, brought about by the arrest of the peristaltic action from above at the seat of stricture; and secondly, by the distension of the atrophied bowel by retained flatus. This state of the rectum seems to be in stricture of the upper segments what a patulous anus and the want of faecal retention is in stricture of the lower segment. It is not met with in all cases of stricture, and particularly in those of rapid formation. It is, however, present as a rule in examples of *chronic stricture*, and should be looked upon as a characteristic symptom. So valuable has this fact been to me in my clinical work that it has often enabled me to confirm a doubtful diagnosis, or to make one in cases in which much doubt had previously existed, and consequently to carry out a line of treatment with confidence."

Later on in the *Lancet*, Jan. 5th, 1889, Mr. Bryant says:—"The digital discovery of a growth or ulceration when the disease is low down, and the ballooning of the rectum when it is higher up, affords with the history of the case and other symptoms ample evidence on which a working diagnosis can be made."

Dr. Durran, of Essex, commenting on Mr. Bryant's paper in the *Lancet*, 19th January, 1889, relates the case of an aged lady, subject to habitual constipation, who was seized with sudden tenesmus, great pain, and a feeling as if there was something in the bowel. The sphincter was spastic, and on introducing the finger the rectum was found to be "ballooned." Enemata were given without relief. A second examination revealed that ballooning still existed, but in the centre of the cavity a mass, apparently of hard faeces, was discovered. This was removed by syringing and the use of the finger.

With reference to Mr. Bryant's statement that "ballooning" is caused by retained flatus, he mentions the fact that

a speculum was used while syringing the bowel, and still "ballooning" continued.

Mr. Burghard, in the *Lancet*, 23rd August, 1890, alludes to 15 cases in which the conditions observed by Mr. Bryant were present. An analysis of these cases shows that there are three conditions with which this symptom is associated.

Chronic constipation with faecal obstruction, of which there were 7 examples.

Stricture, of which there were 5, for the most part malignant and affecting the sigmoid flexure of the colon.

Spinal lesion, which existed in three cases—two of fracture of the spine and one of dorso-lumbar curvature; in these cases the presence of ballooning was only temporary.

Mr. Burghard agrees with Mr. Bryant that it is caused by paralysis of the muscular coat, and states that in two *post-mortem* examinations he found the rectal walls thinned. He concludes by saying that although "ballooning" of the rectum is very suggestive of stricture, yet it cannot be relied upon as any material help to the diagnosis.

Mr. Burghard in his communication makes the statement that Mr. Bryant has brought under the notice of the profession a symptom *hitherto undescribed*. It is a very small matter, but I beg to say that in the lectures on diseases of the lower bowel delivered in Steevens' Hospital, 1883, and published in the *Dublin Journal of Medical Science* for that year, I described this condition in the following words:—

"The sensation communicated to the finger is as if the rectal wall was pasted to the bony pelvis and thus held patulous, and was unable to contract to its normal tubular condition. The lining membrane has a peculiar feel like parchment; as a rule nothing is contained in it but a few small crumbs of dry faeculent matter."

It may be observed that I have emphasised the fact to which Dr. Durran has drawn attention in his case of the old

lady with constipation, that flatus does not escape along the side of the finger or after its removal.

It is difficult to understand how any surgeon making repeated examinations of the rectum for the purposes of diagnosis could overlook this very remarkable condition.

Since it first attracted my attention I have had repeated opportunities of exhibiting it to the members of my clinical class, and having through the kindness of my professional brethren seen most of the interesting cases of rectal disease which have occurred in Dublin for the past ten years, I have had considerable experience of it.

I have seen it under the most varied and dissimilar circumstances, and I must now honestly and candidly confess that I have not been able to attain to a satisfactory solution of the cause of this phenomenon, nor am I able to fix its value as applied to the purpose of diagnosis or prognosis of disease of the intestine higher up.

I have never had an opportunity of observing this state of the bowel *post-mortem*, and I may safely say that my dissections, both anatomical and pathological, have been not a few. Can it be explained by the existence of a valvular condition of the lumen of the bowel above, owing to which the gases are permitted to pass towards the anus, but are prevented from regurgitating along the large intestine, as we know they do in the normal state? Mr. Allingham, the highest authority on such a subject, has been good enough to favour me with an expression of his views. He says:—"I am quite familiar with the condition which Mr. Bryant has called 'ballooning' of the rectum. I think the phrase fairly applicable. I cannot say I have made any special study of the condition, though I have thought about it, and not been able to come to any decision as to its cause. I have seen the distended rectum in all kinds of cases other than acute obstruction. I used to think it indicated obstruc-

tion of the canal high up, and usually acute. It often occurs in children from intussusception of the gut and also from the presence of polypi. It often occurs in cases which are going to be fatal, but I have also seen many recover from it. In such a case, if you put the patient under an anæsthetic and dilate the sphincters (which are always contracted or usually so), and pass your hand into the rectum, you will find the upper opening into the balloon after a little search. The opening is small from spasmodic contraction of the gut. In the case of a girl I found polypus with a pedicle quite an inch long. This I scratched off, and the patient made a speedy recovery. In this case, which was one of acute obstruction, I believed there was intussusception. In a little boy with acute obstruction from intussusception and marked ballooning, after an anæsthetic he was lifted up by the legs and well shaken, with his head downwards. The result was instant cure and disappearance of the ballooning."

Mr. Harrison Cripps says :—" I consider ballooning of the rectum of little value as a diagnostic sign. It is certainly often present where there is stricture high up. On the other hand, I have found it in the rectum without signs of stricture or cancer—healthy, in fact."

I have recently seen, through the kindness of Dr. Scott of Kingstown, a case of well-marked ballooning in which the cavity was divided by a narrowing of the rectum, presenting the appearance of two distinct chambers, communicating by a narrow passage. In this case there was hæmorrhage, but no other positive evidence of malignant disease. In about nine months it developed symptoms of intense malignancy. I have some months ago seen well-marked ballooning in case of injury of the sacrum without any other evidence of disease.

I would invite the members of the Surgical Section kindly

to give me their opinion and assist me to solve two questions—What is the cause of this ballooning? What is its diagnostic value as a system?

My own experience and observation would lead me to formulate the following propositions:—Ballooning occurs in many cases without stricture. Stricture occurs in many cases without ballooning.

DR. BALL discussed the question of dilated rectum, and expressed the opinion that, as a temporary condition, it was common, and without diagnostic significance. When, however, it assumed a permanent character, it was probably often associated with diseases of the rectum, and it is easy to understand how such a disease as malignant stricture would (by pressure on the nerves) lead to a more or less marked paresis.

SIR WILLIAM STOKES expressed satisfaction at Prof. Hamilton having established his priority in having observed the condition of ballooning of the rectum. He thought that it would hardly be looked upon as a diagnostic sign of much value in determining the existence of stricture, as he had observed it in other morbid conditions of the rectum. Sir William Stokes mentioned some cases of malignant ulcers of the rectum on which he had operated, and in which this condition of ballooning was well marked. He thought therefore the diagnostic value of the condition in any special condition of the rectum was questionable.

MR. WHEELER said that he had a gentleman now under his care, formerly a patient of the late Mr. Tufnell; he is suffering from stricture of the rectum, and has this condition now called "ballooning." He congratulated Mr. Hamilton on the interesting communication he had brought before the Society.

MR. W. STOKER mentioned a case of spinal concussion in which the symptom was markedly present.

The PRESIDENT said that he was familiar for many years with the condition of the bowel now known as ballooning, but that condition does not lead to a diagnosis of rectal disease: in ulcer of the rectum the gases cannot escape, and that will account for the distension in those cases. The President said the name ballooning was comparatively new, but the condition was one he had long been familiar with. The fact of such a condition occurring in

spinal cases shows it is not of much help in diagnosis of rectal trouble.

MR. HAMILTON, in reply, said:—I have to thank the members of the Section for the kind consideration which they have given to my paper. Mr. Ball's idea that ballooning is the result of deficient nerve force below a stricture is held by many, but I do not think it gives a satisfactory explanation of the phenomenon. The President does not seem to attach any diagnostic value to ballooning, and yet the opinion of a sound practical surgeon like Mr. Bryant must have weight with a great many. I am surprised to hear the President say that his operations for ulcers and piles were followed by painful distension of the rectum, with flatus. My experience is quite the contrary. I find, since the perfect dilatation and stretching of the sphincter has been adopted as a preliminary to such operations, this is a trouble of the past. The ballooning which Mr. Bryant and I have described has characters more decided than a mere passing distension with flatus.

TRAUMATIC EPILEPSY, APHASIA, AND PARALYSIS OF SIX YEARS' DURATION, TREATED BY TREPHINING: RECOVERY.

By FRANCIS T. HEUSTON, M.D., M.Ch., F.R.C.S. ;

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Consulting Surgeon to the Coombe Lying-in Hospital ;

[Read in the Section of Surgery, January 29, 1892.]

G. T., aged thirty, a discharged soldier, was admitted under my care on March 10th, 1891, with the following history :— About six years earlier, when riding a horse, the animal fell, throwing him violently on his head. His foot catching in the stirrup, he was dragged for some distance, his head being in contact with the ground. He was picked up in an unconscious condition, and removed to the Meath Hospital, where he did not regain consciousness for a fortnight. He remained in the Meath Hospital for about four months, and when discharged his principal ailment was a hesitancy in his speech. He was then retained for a further period of five months in the Military Hospital at Portobello Barracks. His speech then being no better, he was discharged as being unfit for military duty. Within a fortnight of leaving the service he had a severe attack of epileptiform convulsion, which had recurred frequently at irregular intervals—there being sometimes as many as three in a day, and sometimes no attack for a month. He noticed that the seizures were most liable to occur when he was exposed to strong sunlight, being thus more frequent in summer than in winter. He could always tell when an attack was about to come on by the occurrence of bright flashes before the eyes, generally stellate in shape, or a feeling as if some force applied from behind was shoving his head downwards with consider-

able strength. These symptoms might exist separately or together. At other times the attack was preceded by total blindness. After the convulsions ceased a feeling of depression remained, frequently lasting for a week. Since the commencement of the epileptiform seizures he had suffered from continual dull frontal headache. The urine, of specific gravity 1020, contained no albumen, but slight trace of sugar.

The patient presented a heavy and dull cast of countenance, with a lifeless expression of the eyes, such as is frequently seen when mental power is deficient. On being questioned he looked confused, and attempted to answer, but was evidently unable to articulate the word he required, and eventually answered in monosyllables. At other times he would make no effort to reply, although it was evident he fully understood what he was asked. When he spoke his voice was monotonous, with a considerable interval between each word, giving rise to a peculiar clipping of each word, particularly noticeable at the commencement of a sentence. Being asked to name one of his friends standing by, he was frequently unable to do so. Tremors existed in his left hand and arm, and the left side of his face twitched frequently. There was great deficiency in the muscular power of the left hand and arm, but sensation was unimpaired. The knee reflexes were normal, and ankle clonus was absent.

On March 21st, 1891, my colleague, Dr. Swanzy, kindly examined the patient's eyes, and found the field of vision and ophthalmoscopic evidences normal. A triangular scar of the scalp existed in the left occipital region, slightly above which was to be felt a well-marked circular depression of the skull, about half an inch in diameter. This corresponded in position to the second annectant convolution of the brain.

On March 24th, 1891, I made a V-shaped incision with its base above, through the scalp at the seat of the injury;

I found the subaponeurotic cellular tissue intimately attached to the depressed piece of bone; this being removed, a $\frac{3}{8}$ -inch trephine was applied at the seat of depression, and the bone, which was found to be of abnormal thickness and density, there being no diploë, removed. On examining its internal surface it presented no evidence of having been depressed, nor was there any evidence on examining the internal aspect of the skull in the immediate neighbourhood of the trephine hole with a probe. It was, however, clear that the great thickness of the bone in this position was the result of chronic inflammatory changes. The dura mater, which was now examined, bulged into trephine hole, and did not present the silvery appearance of that membrane when healthy; it was evidently thickened, and there was no visible pulsation of the brain. On pressing the dura mater, and thus displacing the fluid beneath it, a slight pulsation was to be felt. An aspirator needle was passed through the dura mater, and some serous fluid removed. A large quantity continued to flow, after the needle was withdrawn, through the opening made by it. The brain pulsation now became distinct and natural. The removed portion of bone, which in the meantime had been preserved between sponges in a warm antiseptic solution, was broken into small pieces, and sufficient nearly to fill the trephine hole was placed in it, room being, however, left for a drainage tube, the end of which corresponded to the opening in the dura mater; the wound in the scalp was sutured, and dressed with zinc sulphite gauze.

March 25th.—The temperature was 99° F., the pulse 76, and the respirations 24. The patient passed a fairly good night, although disturbed at intervals by vomiting. He was given $\frac{1}{6}$ -grain of morphine at midnight. Owing to the dressings being saturated with fluid they were removed. It was then seen that the fluid came from the drainage tube in

a pulsating manner corresponding to the pulsations of the brain, which were very visible beneath the scalp. At 4 p.m. he spoke for the first time since the operation, and with much less hesitation and more distinctly than previous to operation.

March 26th.—The temperature was 98.4° . The patient passed a good night; the dressings were again removed. The pulsation of the scalp was noticed to be much less than on March 25th. Twitching of the left side of the face, and drawing of the mouth to the left, was noticed during the day; but this was not so marked as before operation.

March 27th.—The temperature was 97° . The wound being healed, the stitches were removed and the drainage tube washed out. It was now noticed that the pulsation of the scalp had ceased. The patient expressed himself as being much better and free from headache, while the speech showed marked improvement. From this time the patient continued to improve, and on April 1st the drainage tube was removed and he sat up in bed for some hours.

On April 4th all dressings were removed, and he was allowed to walk about the hospital. There was no pulsation or depression at the seat of operation, the bone replaced in the trephine hole having evidently firmly united. It could not be found that any portion of the skull was deficient. The symptoms he presented before operation were markedly changed, there being but slight hesitancy in speech. He had no difficulty in using any word he required, while his eyes had a bright intelligent look. There was no tremor in his left hand and arm, the muscles of which were rapidly regaining strength, and no epileptic seizure had occurred since the operation.

On April 25th, one month after operation, the patient was sent to the convalescent home, the only abnormality then existing being a slight hesitancy in his speech and an irritability to much noise.

Since writing the above I have seen his wife, who informs me that he has no return of the symptoms, and that his speech has much improved during the ten months which have passed since the operation.

SIR WILLIAM STOKES thought that Mr. Heuston should be congratulated on the result he had obtained in the remarkable case he had just detailed. Sir William Stokes had previously had the patient under his care in the Meath Hospital, but he could not persuade him to submit to the operation of trephining, or rather the patient's wife would not consent to have it performed. Sir William Stokes had shown the case to Mr. Victor Horsley, who happened to be in Dublin at the time, and he also urged on the patient the desirability of the operation, but in vain. Mr. Horsley thought that probably an unabsorbed clot of blood would be found at the situation where the injury was received, but apparently this was not found. The trouble was then probably caused by pressure from the thickened or hypertrophied condition of the bone at the seat of the injury.

MR. WHEELER stated that one of the first cases that he had trephined for epilepsy was a young man, a soldier, who had his skull fractured in the left parietal region by a kick from a mule. A splinter of bone was driven into the brain, causing epileptic seizures. He had detailed the case nearly twenty years ago at the Surgical Society, and would only add, that this patient made a good recovery, and has continued in perfect health, as far as he knew, ever since. The second case was a man of forty years, brought to him by his former pupil, Dr. Kelly, who had been suffering from occasional epileptiform convulsions for three years, and who, three months before he came under his notice, had convulsions every eight or nine days. After careful examination it was ascertained that he had received a severe blow about four years before in the right front temporal region. After a little trouble a cicatrix was found in this situation, and after some consideration he placed the crown of an inch trephine over the bone corresponding to the cicatrix already mentioned. The inner table of the bone removed was much thickened and adherent to the dura mater. Suffice to say rapid recovery followed, and three years ago the man was well. The third case that he could recall the history of at present was a

patient in the City of Dublin Hospital, sent by Dr. Lyster, of Kilkenny, for epileptic fits, very occasional. He was only able to ascertain that the patient had had slight seizures since he had received a blow fracturing his skull close to the left parietal eminence. He suffered from word-blindness, and it was for this that Mr. Wheeler trephined him over the depression; the inner surface of the bone was thickened, and the dura mater thickened and adherent. There was not any abatement of the word-blindness from this operation, and it was not until three or four days after, when observing the dura mater bulge—into which he made an incision, and pus flowed out—that the patient improved, and finally recovered. The next case that he could recall differed from the preceding cases, inasmuch as it occurred in a young male adult, who suffered from a congenital deficiency, and had the operation of trephining recommended for him many years previously by Sir James Simpson. The deficiency existed in the site of the lambdoid suture of the right side; pressure on the edge of this deficiency caused great pain, and symptoms as if the patient was about to have a fit. He had suffered from as many as twenty-five fits and more in the twenty-four hours. After operation the patient lived for three days, and had no return of the epilepsy, although he had one seizure during the operation, and two before. In all the cases that Mr. Wheeler had operated for epilepsy consequent upon injury, he had found the dura mater thickened, and within a very recent date he had the opportunity of seeing Mr. Nixon operate over the site of an injury to the skull; when the bone was removed in this case the dura mater was enormously thickened. Epilepsy caused by injury like the cases quoted was nearly always cured by operation in Mr. Wheeler's experience; but he had over and over again declined any operative procedures in what was termed general epilepsy, the exact site of the "nodus epilepticus" not being defined. There were forms of epilepsy where one might gauge the centre where the lesion was. Dr. Heuston had mentioned that the seventh nerves were engaged; it would be interesting to him (Mr. Wheeler) to know whether the aphasia improved as the muscular power was gained, for his observations led him to conclude that there was an almost definite ratio between the two. There was not any doubt that the patient whose history had been detailed, suffered from aphasia.

MR. SWANZY said:—I regret that Mr. Heuston has not offered some explanation of the symptom of difficulty of speech from which

the patient suffered. In the title of his paper he has termed it aphasia, but in the course of his paper he has, I venture to think, wisely avoided that term. Having had the opportunity of carefully studying the case several times, I formed the opinion that the difficulty of speech was one of articulation, dysæsthesia, rather than aphasia. The man could speak, and had no hesitation in finding the right words, and he understood all that was said to him. He could not write well, for he had never learned to do so. When I first saw him, long before the operation, I thought it probable there was a lesion, hæmorrhage, or rupture of fibres in the pons or medulla, giving rise to the dysæsthesia, twitching of the fingers of the left hand, and some blepharospasm which he had at the time. The result of the operation shows that there could have been no material lesion there; but I still think these symptoms were distant symptoms due to derangement of functions in those parts, rather than in the cortex at the seat of lesion.

MR. NIXON said he thought no hard and fast rule could be laid down as to replacing the bone, as this must altogether depend on the condition of the part removed. In a case in which he had recently trephined the bone was in some places $\frac{1}{2}$ inch in thickness—it was irregularly thickened, which greatly increased the difficulty of the operation, as owing to the density of the skull over forty minutes were occupied in removing the first disc of bone. It was hard, dense, without diploë, and if replaced he believed it would have acted as a foreign body. He thought Mr. Heuston was to be congratulated on the brilliant result he had achieved in the case he brought forward to-night.

The PRESIDENT referred to trephining in such cases, and believed the operation was one too often put off in the absence of serious symptoms. Serious symptoms often develop suddenly. The President referred to a case alluded to in his address on “Head Injuries,” at the opening meeting of the Surgical Section of the Royal Academy of Medicine last Session. The patient, a gentleman, had symptoms which, in his opinion, pointed to the necessity for trephining; but his (the President’s) hands were tied at consultations, and the operation was not permitted. The patient’s condition became much worse, and at a later period a portion of bone exfoliated, and the patient recovered.

MR. HEUSTON said the manner in which the bone was replaced was as follows:—The bone being broken into small fragments by a bone-forceps, those fragments were placed in the trephine hole,

resting on the dura mater, with the intention that they should unite and further the process of union of the skull. This was done, not alone owing to the fact that it was so highly recommended by different authorities, but also that he had, in former cases of trephining, ample opportunity of observing how fully they attained the intended object. In one case, where repeated operations had been performed, in the first operation no bone had been introduced, and a permanent opening in the skull remained, whilst in the later operations bone had been placed in the trephine holes, and after a short period no external evidence of the operation remained. He was glad to hear that Sir William Stokes had Mr. Victor Horsley's concurrence with him in the advisableness of operation. In reply to Mr. Wheeler's observations respecting the implication of the seventh nerve, he would refer him to the spasmodic contractions of the left side of the patient's face as indicating irritation of that nerve. The patient's hearing was not, however, implicated. His opinion as to the nature of the case agrees very much with that expressed by Mr. Swanzy, except in that there were, in his opinion, distinct evidences of aphasia as noted in the case. He was of the opinion that the chief symptoms were due to an osteoplastic osteitis, with a pachymeningitis externa, giving rise to a pressure on the brain in a twofold manner—first from the thickening of the bone and dura mater, and secondly from an increase of the subdural fluid; otherwise he could not understand the improvement in the case after operation, or the absence of pulsation of the brain when the bone was removed.

THE TREATMENT AND ORIGIN OF HAMMER-TOE.

BY ROBERT LAFAYETTE SWAN, F.R.C.S.;

Surgeon Dublin Orthopædic Hospital, and Steevens' Hospital.

I ASK your attention to this subject for a short time for the following reasons:—

That it is an affection which in exaggerated cases produces the utmost misery and suffering. That the instances which exist are very numerous. That the literature of this subject is in books of standard reference in surgery singularly scanty, and the directions given for its treatment I believe inefficient: and lastly, that while in certain papers which within the last few years have dealt with the subject, the conditions which co-exist with the deformity are described, no attempt, so far as I can discover, has been made to arrive at an explanation of the true origin of this distortion.

For several years past my attention has been directed towards these points, and I venture to submit the deductions, both practical as to its treatment, and theoretical as to its origin, to the consideration of the academy.

I cannot better illustrate the distress occasioned by this distortion than by reading the following extract from the letter of a gentleman:—"My wife, aged 24, has had as long as she can remember a bent condition of the second toe on each foot, and latterly she has endured the greatest misery from it. She is fond of riding, but cannot wear a boot. The top of the toe is flattened and very painful, and seems, no matter what kind of boot she wears, to always press violently against the sole. The nail is thickened and loosened, and a slight discharge comes from under it. The upper part of the toe,

if she tries to wear a decent boot, becomes easily inflamed. Though actively inclined, she can neither walk, play tennis, ride, nor take any active exercise, and it renders her life quite wretched." These casts were taken from the feet referred to before treatment.

On reference to standard surgical works, it will be found that no notice is taken of this affection by Gant, Fergusson, or Agnew. Gross classes it with hallux flexus, and considers it easily cured by section of the flexor tendons. Now, it is completely distinct from hallux flexus, which occurs only in males, and is an acquired affection, whereas hammer-toe is common to males and females, always I believe congenital and often hereditary. In the article written by Dr. Little for Holmes' system of surgery, he also advocates section of the flexor tendons. Mr. Erichsen describes it in the following words:—"In this complaint the proximal phalanx is on its normal level, or slightly drawn up. The two distal are bent down at an acute angle, the apex of which is formed by the articulation of the first with the second. This contraction more commonly affects the second toe, is often symmetrical in both feet, and is frequently a source of great inconvenience and even permanent lameness. It appears to be due to contraction of the digital prolongation of the plantar fascia, and is best remedied by dividing this subcutaneously." In the French medical society, in 1888, M. Terrier read a paper on the treatment of hammer-toe by resection of the first interphalangeal joint. Messrs. Bran and Charles had previously reported similar cases. Mr. Adams recommends the section of the lateral ligaments, the shortening of the inferior fibres of which he considered the cause of the deformity. He probably meant the fibrous bands connecting the sheaths of the tendons with the lateral ridges on the phalanges. Mr. Anderson, in 1889, also gave a very clear description of the conditions that exist, and the means to be adopted for

rectification. Most men are disposed to be led by generally accepted doctrines, and it is a tendency which has its wholesome deterrent uses. I followed those doctrines for some years, divided the flexor tendons and the ligamentous structures, with occasionally some little improvement, and perhaps in slight cases with success, but the mortification of failure led me to seek some surer method of dealing with this condition. In 1885 I removed the head of the first phalanx, after a thorough and repeated examination of the anatomy of the distortion and trials on the dead subjects. The rectification was immediate and complete. I have since operated on a large number of cases in which the distortion was often symmetrical. The results were invariably all that could be desired.

Through the kindness of Professor Frazer I am able to produce this dissection of hammer-toe. The first phalanx is in its natural position. The second and third are flexed. The sheath is opened and the tendons are loose, and yet we see the deformity remains unaltered. The head of the first phalanx is only partially in the articulation, and is marked by a transverse ridge dividing the articular from the non-articular portion. The non-articular portion shows a deficient incrustation of cartilage, and is covered by the extensor aponeurosis which plays over it. The fibres which attach the anterior part of the first and proximal extremities of the second phalanges to the sheath are shortened, and appear to be at this stage of the dissection the immediate cause of the distortion. But we know that all structures, such as here exist, rapidly adapt themselves to assumed positions of the skeleton. This is well seen in diseases of joints, and in many congenital or acquired deformities. Let us take, for example, the ordinary equino-varus. Here we have ligaments, tendons, fascia, blood-vessels, nerves, and skin, all shortened, and it would be equally logical to state that one of those structures was a

factor of the condition as another, unless we could clearly demonstrate it to be so. We have in this deformity the ligaments, the sheaths of the tendons, the tendons, the subcutaneous slips of fascia, the skin itself equally shortened; but these are, I believe, the result, not the cause of the condition existing, as the removal of a portion of the skeleton produced ready rectification. I was led to believe that it alone was in fault, and if content with the satisfactory result of the application of such knowledge, all was easy; but here I was confronted by the problem—Why should the second toe be invariably so affected? There are certain facts which point to its elucidation, which I shall briefly relate.

Apart from progression, the functional uses which the toes may serve are varied. The Polynesians, Dyaks, negroes, and certain tribes of Indians, have an extraordinary dexterity in grasping objects with their toes, or between the first and second toe. There are tree dwellers amongst the Indian tribes, residing along the Amazon rivers, engaged in the collection of India-rubber, who have wonderful powers of grasping objects with their toes. It is stated by Hartmann that among many of those people the great toe, even in rest, is separated from the second, and the distinction between the foot of man and that of the anthropoid apes is less marked. There is a tribe in Central Africa, named Makrata, in which this is extremely evident.

Dr. Buckmer (*Conférences sur la théorie Darwiniensis*) has also given cases of the use of the foot as a prehensile organ in man. The great toe of all anthropoid apes is shorter than the second, and extremely small in some. Through the kindness of my friend, Professor Cunningham, I am able to show this exemplified in the feet of the chimpanzee, the orang outang, and the gibbon. So that really in some of these the action of the foot as a prehensile organ must, in powerful efforts, be more of a grasper by the flexors of the toes than

by the opposing power of the great toe to the others or to the sole. Man alone is characterised by the possession of a great toe longer than the second, although this appears to be by no means a constant feature. In Greek sculpture the second toe is represented as longer than the first, as is seen in the statues of the Hercules Farnese and the Apollo Belvidere. This was probably copied by the Egyptians, and originally derived by them from the negro. Here we see evidence of a reversion to a lower type of foot. Professor Flower examined the feet of many hundreds of children who had naked and therefore undeformed feet, and did not find a single instance of the second toe being longer than the first; but if we measure the disarticulated bones of a hammer-toe, we always find it longer than the first. The anatomy of the second toe shows that it is admirably adapted for functional usefulness. It has two dorsal interrossei which, as proved by Duchenne, serve to extend the first phalanx, and help to flex the second and third; but they also must contribute to lateral motion. The extensor aponeurosis is attached to the second and third. The flexor attachments to the second and third phalanges are most powerful. In the case of an accidental mutilation it serves as an admirable substitute for the wonderful versatility of motion afforded the great toe by its seven special muscles. Here is an illustration of the development of the second toe in such a case; but this endowment must mean more than merely to meet the necessities of an accidental mutilation.

In the works of most of the older anatomists we find that the second toe is depicted as longer than the great toe. This is particularly evident in the plates illustrating the works of Haller, which I examined through the kindness of my friend Dr. William Frazer, and which are so generally accurate. It has been asserted by Weissmann that mutilations are not hereditably transmittable. The idea, which is seriously stated

by many writers, that this distortion is a mutilation—that is, produced by an ill-fitting boot—may be at once discarded. It has no such origin, and is, I believe, always congenital, although the inconvenience of the conformation does not arise till the foot has been used, and it may certainly be aggravated by ill-fitting boots. It is also often hereditary, and most frequently is found in the second toe of both feet. This would, if Weissmann be correct, bear out the view that it is not a mutilation. We are not without evidences in the human foot of being, in the process of evolution, closely allied to a lower type. The anthropoid apes have the sole of the foot turned inwards, and rest on the outer side of the foot—in fact, they have naturally an equino-varus. And here again in such a deformity we see a reversion to a lower type. Such a condition, I am informed by Professor Fraser, is proper to the embryonic foot even to a tolerably advanced period of development. The same distinguished embryologist has informed me that the fibula in the fœtus does not descend below the level of the internal malleolus, as he has demonstrated by numerous sections of the fœtal tarsus. This is a complete similitude to the fibula of the anthropoid apes, and is an adaptation of structure to function, as the arrangement affords more freedom of motion, but less strength to the ankle-joint, with the result that man, when developed, enjoys greater power for the maintenance of the erect posture, and the exceptional privilege in the animal series for the acquirement of a Pott's fracture. From this combination of facts, very imperfectly expressed, I venture to suggest that the deformity known as "hammer-toe" is a partial reversion to a lower type of foot, and that the osseous elements of the grasping toes are developed out of all proportion to the soft structures.

MR. HAMILTON considered the Academy was indebted to Mr. Swan for his paper, which was characterised by originality and research, as well as by practical usefulness. The surgeon was apt to follow routine, and he had himself on many occasions divided the flexor tendons for this affection. He had no doubt, from what he now saw, such teaching was erroneous. He had himself been struck with the functional usefulness acquired by the toes, as illustrated by the divers in the Bay of Naples.

MR. T. MYLES congratulated the Section on the pleasure and benefit they had derived from listening to Mr. Swan's very exhaustive paper. The speaker, however, disagreed with some of Mr. Swan's statements, and considered that his deductions were hardly warranted in their entirety. For instance, the second toe, normally, was not shorter but always longer than the first. Mr. Myles pointed out that in the condition under discussion the position of flexion of the second toe had the effect of removing the broad anterior extremity of the phalanx from between the first and third, and was, in fact, nothing more than a spontaneous effort to diminish the effects of the crowding together of the toes by tight boots. If Mr. Swan's theory were true, it ought to be equally common amongst people who do not wear boots as amongst those who do.

MR. WHEELER thought that the last speaker argued from the particular to the universal. In any cases he had seen of hammer-toe he was led to believe that it was congenital. On two occasions he excised the joint and placed the toe in the straight position.

MR. PRATT referred to the evolutionary development which occurred in the feet of birds as having some bearing on Mr. Swan's explanation of the origin of this affection.

TREPHINING (1) FOR CEREBRAL CYST; (2) FOR UNCOMPLICATED WORD-BLINDNESS.

By WILLIAM IRELAND WHEELER, M.D., F.R.C.S.;

Surgeon City of Dublin Hospital.

[Read in the Section of Surgery, April 8, 1892.]

IN the latter part of the year 1888 I was requested to see a lady from the south of Ireland, stated to be suffering from an "ascending cortical degeneration." Her history as I received it is as follows:—In the summer of this year (June) she was subjected to some mental and domestic annoyance, and on one morning about one hour after breakfast she became partly unconscious, her speech was markedly imperfect for some time, and never regained its former perfection. She had loss of power in her right facial muscles, and in her right hand and arm and leg there was impaired motion. This lady rallied after an hour or so, and recovered sufficiently to enable her to visit the Glasgow Exhibition, and, as I am informed, did not express displeasure at her trip.

As she was not improving, but rather the contrary, I was consulted about the end of October, or beginning of November, in the year 1888. The train of symptoms at that time nearly corresponded to what I have recorded above, except that her speech had become more impaired, and the partial paralysis of her leg and arm more apparent.

The subject of this inquiry was about forty-four years of age, of medium stature, sallow complexion; her catamenial periods were regular. She had borne one child, a male—now about twenty-one years of age. Her appetite was good, and her digestive system was unimpaired; her loss of power has been described. The dynamometer registered marked

difference of power between her right and left hands; there was no loss of sensation by the æsthesiometric test. There was an appreciable difference of temperature between the impaired and unimpaired limbs as measured by a surface thermometer. Her hearing was normal, as also her vision. Her response was slow and with hesitancy; she generally found the right words in reply, and yet occasionally misapplied a word; she could read the newspaper imperfectly, and could generally spell a word that she did not know, yet not pronounce it; when named she could repeat it; her pantomimic speech was not much impaired, if at all. She was unable to write perfectly from dictation, omitting letters at the end and middle of her words, and when writing from a copy with similar results, one example will suffice. The late Dr. Corley, who saw her with me in consultation, and asked her to write as follows:—Tell Mr. Wheeler to call to-morrow. She wrote—Tell Mr. Doe to ca tomo-rw. She could, however, write her name perfectly and correctly. Not unfrequently she complained of shooting pains in the left side of her head, and sometimes upon the right side. Occasionally there was some slight loss of appetite; upon some occasions she walked better than others—she might walk very well upon a Monday, and on Wednesday the drag in her leg would be much more marked. For reasons hereafter, I advised operation, but in this the late Dr. Corley, whom I had met in consultation, could not coincide.

Dr. Fitzgerald, who examined her eyes, could not find anything wrong with vision. I had previously examined her sight with similar results. Notwithstanding the high opinion I entertained for Dr. Corley's sagacity and diagnostic skill, a week after I was constrained to reiterate my opinion, which I believe would have been acceded to by her sister, who was in constant attendance, but for the advice of a practitioner who saw her in consultation in the country, and who, happen-

ing to be in Dublin at the time, through my courtesy saw her with me, and stated to her sister, as repeated to me, and tapping his hat, not then upon his head, said that fluid might as well be taken from beneath it.

Matters were pretty much the same in the following February, 1889. As I had to go to London for two or three days, I suggested to the lady and her sister to have another opinion, and they gladly assented. An examination was held by a gentleman whose name is a sufficient guarantee for his accuracy in cerebral affections and cerebral lesions. The arrangement which I suggested was, that there should not be any consultation, but an opinion expressed without conference. The opinion advised that change of air and tonic treatment should be tried, &c. I then stated that I had strongly expressed the necessity for operation, but his opinion was not materially altered. Upon her visit to London it was ascertained that she was suffering from hemianopsia. On her return I made further examination, and for the first time I ascertained she was suffering from loss of smell on the left side. I was more confirmed in my opinion, and again expressed it. I ordered her to Bray for change, and prescribed a tonic, from which there was not any improvement. On the contrary, the patient grew worse, both in her articulate and written speech, and in her gait, and what may be called apperception. Finally she got paralysis of both lower and upper limbs, total aphemia, and pantomimic speech was eventually lost. It was now nearly six months since I had advised operation, and even now as a *dernier réssort* I still recommended it, and at the eleventh hour it was consented to.

Upon Easter Sunday, 1889, the patient being under the influence of chloroform, assisted by Dr. Duffey and Dr. Corley and Dr. Harley, I trephined over the left parietal eminence. Immediately upon the bone being removed, the membranes

bulged out even beyond the surface of the calvarium. I shall not detail the operative measures, save one, particularly—that is, the incision in the dura mater. I usually make this circularly round the margin of the trephine opening, and not too close to the edge of the bony aperture, and not completely round, but leaving interspaces. The advantage of this method over the crucial incision is that, when the membrane is being readjusted the firmest pressure is over the centre of the brain exposed in the aperture, and where the brain most usually protrudes. By not going too close to the edge of the aperture, it leaves sufficient room to apply sutures through both sides of the severed dura mater. This is worthy of notice, and not unimportant in cases where it may not be desirable to replace the bone. Upon raising the membrane, and exploring with a syringe needle, I obtained fluid (here produced). I regret that circumstances have prevented Dr. Duffey being present here to-night, to whom I stated that fluid would be obtained, and who could tell that I had written beforehand what I should most likely find. My statement was justified, no less than 4 ozs. of fluid having been concealed.

The result of the operation was encouraging. The patient, within less than half an hour, spoke slowly; an example will best explain. Her husband entered the apartment, holding a telegram in his hand; she exclaimed, “Whom is that telegram from?” he replied, “It is from that Mr. Blank.” She replied, “Is that Mr. Blank, of——; what does he want?” Within one hour her left arm recovered power, and the right arm as well as it was previously to the total loss of power. Towards evening of the day after Monday her left leg partially recovered, her right leg did not improve, and pantomimic speech was present until Thursday. She died upon Friday, five days after the operation, about six months after I had first recommended it.

I had intended to allude to a case of pure uncomplicated word-blindness—which affection is not often met with—as several points in this case could with advantage be contrasted with the first case, but time would not permit—the contrast must be postponed. Suffice it to say that I trephined in the same region, and evacuated pus, with the result of an excellent and continued recovery now two and a half years ago.

In considering the unfortunate case which I have recorded, and which was lost through procrastination, it had occurred to me that, on account of the pain in the side of her head and ear, possibly the want of power in the limbs might be caused by irritation in the medulla oblongata—perhaps a local phlebitis of the vessels of the medulla as the acoustic vein joins the veins upon the anterior of the medulla oblongata—and that the right facial paralysis was caused by the irritation of the centre of the seventh nerve, and that the defect in her speech was likely due to the fibres of the left restiform body running upwards. But this idea was early dissipated from my mind, and upon further reflection I localised the lesion at the site of the operation, and in coming to this conclusion I was much assisted by the train of the phenomena, and by the exclusion of many lesions suggestive as causing the disorders. That it was not purely motor aphasia, even in the very advanced stage, was evident. There was no bulbar paralysis, for there was no difficulty of deglutition, and no labio-glosso pharyngeal paralysis.

I did not suspect a tumour—first, because of the absence of optic neuritis, although optic neuritis is frequently absent although a tumour may exist; secondly, by reason of the suddenness of the attack and the rally; thirdly, because I have found that in fluid collections in the brain, associated with aphasia, change of position not unfrequently relieves pain and occasionally improves articulate speech, but not

written speech. Besides aphasia and its various modifications, and agraphia with its modifications, and amnesia, this patient also was the subject of apperceptive aphasia. As this lesion is situated in the surroundings of the branches of the Sylvian artery, it also was a source of help in localising the site for trephining, which was not arrived at without careful thought, together with much toil and reflection.

DR. HARLEY said, as one of those who was present at this operation, and witnessed the very marked result of it, he might be permitted to say a few words—chiefly in corroboration of what Mr. Wheeler had described to them. As it had occurred that this patient had been on several occasions under his care during temporary absences of Mr. Wheeler, he was fully aware of the state she was in before he saw her on the table, and could better realise the sudden return of consciousness and motor power, not only in remarking in her husband's hand the telegram, and asking from whom it came, but before he (Mr. Harley) left the room she had put her hand up to her head. The late Dr. Corley was present, and he thought his (Dr. Corley's) delight, when he saw the fluid in the aspirating needle, was quite equal to that of Mr. Barton when he saw the pus, in his case of "Aneurysm or Abscess," just read by him.

MR. WHEELER, in reply, could not state exactly what the lady died of, as an examination was not granted. Her death, he believed, was reported as from syncope. In his experience convulsions were not common in cysts of the brain, as above described.

TREPHINING FOR MENINGITIS.

By J. S. M'ARDLE, F.R.C.S.;

Surgeon and Lecturer on Clinical and Operative Surgery, St. Vincent's
Hospital, Dublin.

[Read in the Section of Surgery, May 20, 1892.]

IN bringing this subject under your notice, I have two objects in view—1st, to place on record a case which I will hereafter detail; and 2nd, to elicit your opinion in reference to a line of treatment which suggests itself to me for the relief of some, at least, of the forms of meningeal inflammation. We have all seen cases of syphilitic tubercular and traumatic meningitis die, and not infrequently does a meningeal effusion terminate life in cases of alcoholism. Now, elsewhere, in areas open to surgical interference, in cases where tension is tending to a fatal result, local or general, we aim at relief of that tension by incision and free drainage, and I think the time has arrived for applying this surgical principle to the last of the closed cavities, to be handed over to the tender mercies of the surgical side of the profession. For long the physicians could not see their way to surgical intervention in cases of serous or other collection in the peritoneum, pleura, or pericardium; but the advances of modern surgery have emboldened them, and far from in any way impeding our advance, they are the first to see the necessity for all justifiable operations on these regions.

We still linger by the cradle of brain surgery, and let us hope that with the aid of the highly skilled diagnosticians whom we now boast, we may ourselves witness the advantages of its adolescence.

The case I am about to detail is an instance in which I felt that vigorous surgical interference was demanded but would not be permitted, and you will agree with me that life might have been saved if my suggestion had been carried out.

CASE I.—James Egan, aged thirty-eight years, was crushed by a horse against the side of his stall, sustaining injuries to his chest and back. After a time he complained of severe pain in the back and right side of head, gradual loss of strength, and general mental weakness. He came under my care in the following condition on May 10th, 1890 :—

He was apathetic, answered questions very slowly, and all response to questions of any kind was very tardy. His pulse was slow (50 per minute), soft, and full. Temperature sub-normal, ranging from 95.6° to 97.8° , occasionally in the evening reaching 98° . From the first, emotions, if he experienced any, affected only his inner self, his blank face never bearing any evidence of their existence. He was able to move about, but his movements seemed purposeless. All through he took nourishment freely, and the urinary and digestive systems were normal.

All our efforts at relieving the distress by blistering and the exhibition of mercury proved fruitless, the patient going from bad to worse, first losing control of his legs (a condition necessitating confinement to bed), then a similar condition of the arms set in, so that he was never able to assume a desired position without aid.

The power of co-ordination was completely lost, while muscular action was not interfered with. If asked to put his hand to his head or to reach for anything, the result was painful to witness. The poor fellow would vainly endeavour to control the movements of the limb, which generally went in every direction but the proper one; and usually when the other arm was requisitioned to aid the unsteady

member, a series of eccentric bimanual movements resulted—rarely, indeed, ending in the performance of the desired act. This state of things continued for some days, then deglutition and respiration became affected; and, notwithstanding all our efforts, he died on May 26th, 1890.

The *post mortem* revealed a thickening of the membranes of the upper part of the cord; and at the base of the brain, effusion under the tentorium, compressing the cerebellum and medulla, was found to be the cause of death.

Now this is a case in which there was no direct cranial injury, but nevertheless death resulted from intra-cranial pressure, and a trephine opening into the cerebellar fossa would have relieved the symptoms and probably saved life; and my suggestion is, that what we do for intra-osteal pressure elsewhere should be done in such cases whether arising as the result of tubercular, syphilitic, or other meningeal inflammation. I have examined the membranes of the brain in fatal cases of tubercular meningitis, and although the majority showed diffuse meningeal thickening, I can recall several in which only a few small grey nodules were found, such as are discovered in cases of peritoneal tuberculosis which end favourably after irrigation. A similar result should, at least in some cases, follow interference in the cases under discussion. The case I have just related occurred in a syphilitic subject, and, no doubt, his constitutional condition at the time of the accident predisposed him to the affection from which he ultimately died; but the *post mortem* disclosed only a removable cause, and one which, but that my hands were tied, would have been removed, and very probably with success.

The next case which I am about to relate comes within the category of inflammatory troubles, although some would say that it was one of subdural hæmorrhage.

CASE II.—William Green, aged forty, was admitted to Saint Vincent's Hospital on the 3rd December, 1890, when

the following history was elicited:—On Saturday, November 15th, he fell into the hold of a vessel, striking the left side of his head; he was removed in an unconscious condition to Sir Patrick Dun's Hospital, where a wound over the left eye was dressed. In a few hours consciousness returned, and he went home the same evening. The next day being Sunday, he rested, and on Monday resumed his work as coalporter. All went well with him until Monday, December 1st, when he felt disinclined for work, had some sickness of stomach, and noticed his left hand weak. During the evening of that day he experienced severe pain in the top of his head, and the weakness of the arm increased. On Tuesday, sixteen days after the accident, he found the left leg becoming powerless, and the following day he came under my care. He then had partial paralysis of left arm and leg. There was a scar over the left eye; pulse, 54; temperature, 96·5°; pupils normal, and respond to light. Although he seemed stupid he answered quickly and accurately. I had his head shaved, and the most careful examination did not disclose any external evidence of injury on the right side. It was evident from the slow pulse and sub-normal temperature, as well as the paralysis, that compression of the brain was present.

The history (early unconsciousness, then an interval, and now rather rapid occurrence of paralysis) pointed to secondary hæmorrhage; but the fact that pressure symptoms so gradually increased convinced me that, although a hæmorrhage might be the primary cause, inflammatory trouble must have supervened. Ice was applied to head, and mercury administered; but the symptoms grew worse, until, on the morning of the 5th (fifty hours after admission), the following note was sent to me by Dr. Garret Hickey, to whose able assistance are due many of my successes, both in private and in hospital practice:—

"Dec. 5th.—Green slept pretty well during the night; but at 6 a.m. I was called, to find that he had just had a convulsive attack, which weakened him very much. At 7 20 I saw him again, and learned that in the interval he had had several, only 5 to 10 minutes elapsing between each." The attack which caused him to send an urgent message for me he describes as follows:—"The fingers of the left hand contract firmly into palm. The hand is then flexed on the forearm; pronation then takes place while the arm is being elevated straight up from the body. About this time deviation of the eyes to the right occurs, and the face soon follows in the same direction, contraction of left face being marked (for the first time gurgling in the throat and difficulty of breathing are noticed); the arm now drops, not suddenly, but by short jerks, and the left leg works convulsively." He winds up this hurried, but accurate and expressive note by saying, "I would wish you to come as soon as possible. He is perspiring freely, and is much exhausted after the exertion." I hurried to the hospital, saw a repetition of the performance, so well described by Mr. Hickey, and a picture of greater distress I never witnessed. Fortunately, the patient was totally unconscious, and had been so for some time. The temperature was still subnormal, and the pulse varied from 45 to 50 per minute. In the last attack which I saw, and the last we allowed him to get, respiratory difficulty was very pronounced, the poor fellow becoming cyanosed. I felt that further delay would be criminal. Removing him to the theatre, I at once removed the piece of bone exhibited (Fig. 1). The dura mater projected into the wound, looking very dark. On incising it, a greenish, serous fluid gushed out, and on raising the membrane, after detaching it three-fourths of the way round, I found a clot adhering firmly to its inner surface. This I scraped

away. After thoroughly irrigating the parts with boracic solution, I sutured the dura mater, laid a medium-sized drainage-tube across the wound, passing deep sutures to fix the scalp flap. For some time there was a copious serous oozing, which necessitated frequent change of the dressings; but late in the evening drainage seemed complete, and at 3 a.m. on the 6th consciousness returned, the patient raising at the same time the arm which had been paralysed, and saying to the nurse in charge, "Begor, I've the use of myself, ma'am."

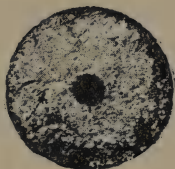


FIG. 1.

From this time recovery was uninterrupted, and on the 14th—that is, eight days after operation—the patient was about, the wound being soundly healed. The ultimate result of the operation you have witnessed. The patient you have examined has never lost one day's work through ill health since his dismissal from our convalescent home, one month after operation.

In determining the position at which to apply the trephine, I was guided chiefly by the starting point of the spasms, and I cut down in the area which gave me the greatest chance of reaching the hand and arm centres, as the convulsive seizures started in these centres, and spread to those of the face downwards, and to those of the leg upwards.

This photograph shows the exact situation in which

I trephined, as you may see by comparing it with the patient's head. Now this was the exact spot at which an adherent clot was found, and when we come to study the next photograph, we find that this area corresponds exactly with the supposed situation of the hand and arm centres, and one can easily explain the course of the case from gradual loss of power of the hand and arm to a loss of power of the lower extremity, and ultimately to a complete left-sided paralysis. Extension forwards of the pressure would explain the conjugate deviation of the eyes towards the side of the lesion, and extension backwards would account for loss of sensation and of consciousness.

In this case a small blood clot was, no doubt, the starting-point of the trouble, but a localised meningitis was the condition for which, after consultation with my colleagues, I trephined this man. The operation confirmed the diagnosis, and the result which you have witnessed fully justified the undertaking. Before operation I was careful to mark the line of Rolando's fissure, but when the patient was anæsthetised and brought into the theatre, I did not like the direction of the line marking the fissure, and on re-measuring the fronto-occipital line I found that on the previous occasion I had placed the upper end of that line half an inch too far forward, as in the first marking I had commenced the measurement by placing the tape at the root of the nose, and thus in subdividing the distance a difference of half an inch occurred. Now, as the clot removed was adherent to the dura, a faulty marking might have led to less favourable ending. I merely mention this error, as it is possible others could fall into a similar one, and the warning may be of service.

During the operation, Mr. Patteson, who had charge of the patient, called my attention to a curious effect of the working of the trephine, the explanation of which I would be glad to

elicit. Before operation and during the intervals occupied in examining the depth of the trephine wound, the pulse never was above 54 per minute. The moment I exerted pressure the pulse went up to 80, and this change occurred on every similar occasion all through the operation.

Before definite symptoms of meningitis showed in this case my notion was that secondary effusion of blood occurred during some effort at lifting heavy weights, and I was deliberating over which of the hæmorrhagic areas described by Kronlein, and depicted in the accompanying woodcut, Fig. 2, it would be advisable to trephine should no improvement take place.

That in perforating the skull in the situations selected one strikes important meningeal branches is shown by

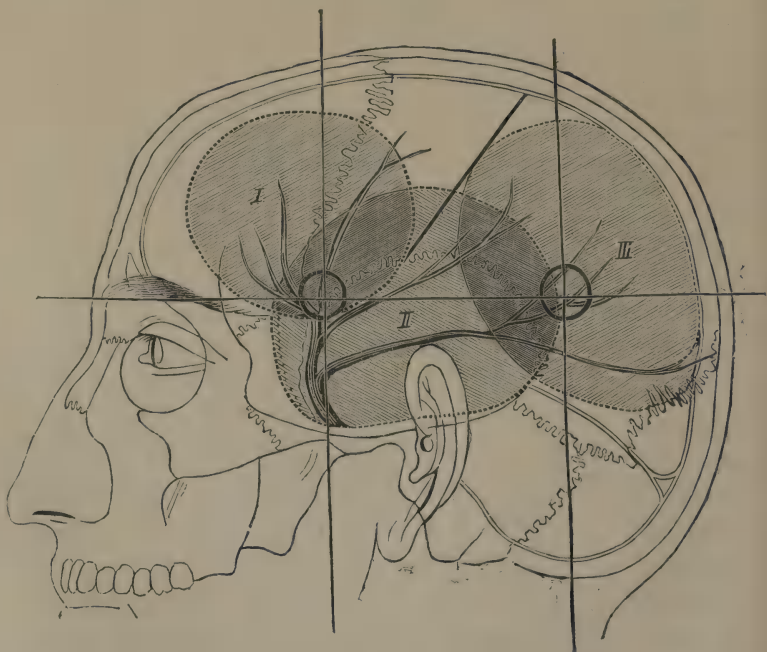


Fig. 2.

the pieces here depicted—Fig. 3—which I removed by a trephine of only 8 mms. One comes on the anterior, the other on the posterior division of the middle meningeal artery. In the absence of definite local symptoms, and with symptoms of meningeal hæmorrhage, I would be inclined to adopt Kronlein's suggestion and seek the clot in either of the positions marked out, as a trephine of 16 or 20 mm. would expose with certainty clots or effusions of any size if applied at the points indicated.

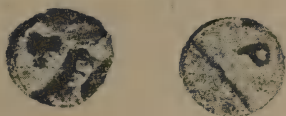


Fig. 3.

As the diagram is instructive I take the liberty of laying it before the Academy, in the hope that some members more versed in the anatomy of the cranium than I am may express an opinion on its merits or demerits as a guide in operations on that part.

The points of the case I desire to recall are—1st, there was unconsciousness immediately after the accident lasting some hours; 2nd, there was no trace of injury to the right side of the scalp; 3rd, there was evidence of a wound over the left eye; 4th, the patient worked at a laborious occupation for well-nigh three weeks after the injury; 5th, death was imminent from laryngeal spasm immediately before operation; 6th, serum, not blood, was the immediate cause of pressure symptoms; and 7th, the membranes were thickened and showed that meningitis had been set up.

Some years ago, when the question of surgical treatment of intra-cranial hæmorrhage was so ably stated here, the term "trephining-epidemic" and such epithets were used,

reminding one of similar expressions regarding operations on the abdomen when the surgery of that cavity was forging ahead in spite of every obstacle. To no chapter in the history of our art can the student of surgery turn with greater pleasure than to that on the modern surgery of the brain, and none can he peruse with greater profit; and I may be forgiven if I say that I am proud that the impress of Irish surgery is there indelibly stamped, and that by members of this Section of the Academy. Feeling that I only add a brief and halting record to this glorious page, I can but crave your pardon for thus occupying your time and to such slight purpose.

The CHAIRMAN (SIR WILLIAM STOKES) observed that, in his opinion, Mr. M'Ardle's communication was one of great importance in connection with the subject of brain surgery, which for some years has been, and still is, engaging the attention of surgeons. He agreed with Mr. M'Ardle's remarks in reference to the operation of trephining acting as a preventive measure to the occurrence of inflammatory troubles subsequently supervening. His experience goes a long way to confirm the views of the older surgeons in reference to the value of trephining as a means of preventing inflammatory complications after cranial injuries. Mr. M'Ardle's remarks as to the necessity of accurately determining by careful measurements the appropriate situation where the operation should be performed were of the greatest importance.

MR. TOBIN referred to a case somewhat similar to that of Mr. M'Ardle's, in which he got a successful result after trephining and draining away a quantity of serum underlying the dura mater, and gave it as his opinion that tubercular meningitis might, in a considerable number of cases, be successfully treated if they were freely drained at an early stage.

MR. J. H. SCOTT said two minor points in this case appear of some importance. First, why was mercury administered? He considered mercury had no effects on any inflammatory effusions except those of a syphilitic origin. Secondly, the apparent osseous closure of the trephine opening without replacement of the removed

bone. He thought it probable the osseous development was due to some of the superficial osteoclasts adhering to the detached dura mater.

MR. M'ARDLE was sorry that he could not elicit an explanation of the change of pulse noted by Mr. Patteson. The speaker's opinion was that the working of the trephine stimulated those centres which the pressure of the fluid prevented from action without strong stimulation. In the case detailed by Mr. Tobin the chief symptoms were the same as in the case noted, but the deviation of the eyes was in the opposite direction. The difference is easily explained in Mr. Tobin's case—the pressure was not sufficient to check the action of the centres on the injured side, but the irritation of these centres would have the effect noted. Mr. M'Ardle, replying to Mr. Scott, said he used mercury in this case because in very many non-syphilitic inflammations, meningeal and otherwise, great benefit resulted. In reply to Dr. Cox, Mr. M'Ardle said he would not operate on cases in which the meningeal trouble was secondary to some rapidly-progressing fatal affection, or in chronic alcoholism where fibroid degeneration of important organs was present. The speaker begged to thank Sir William Stokes for his support of the opinion that trephining should have been tried in the first case related.

ON THE USES OF ELECTROLYSIS IN AFFECTIONS OF THE MALE URETHRA.

By C. YELVERTON PEARSON, M.D., F.R.C.S. ENG.;

Professor in Queen's College, Cork;
Examiner in the Royal University of Ireland, &c.

[Read in the Section of Surgery, May 20, 1892.]

THE subject of the uses of electrolysis in affections of the male urethra is one which, I trust, may not be devoid of interest to the Surgical Section of this Academy—more especially as it has not received that amount of attention in this country or Great Britain to which I believe it is justly entitled—and I trust that the results of my somewhat limited experience may throw some additional light on the class of cases for which it is most suitable, and may stimulate some of my professional brethren to give it an impartial trial.

I believe the fact that electrolysis in the treatment of stricture and allied affections has not met with much favour amongst our leading surgeons, is due, in the first place, to a natural prejudice against novelties, and a conservancy in favour of older and more tried methods; in the second place to a distaste for mastering the necessary details; and thirdly, perhaps there exists a feeling that the adoption of this mode of treatment might have a tendency to drive such cases to the specialist in electricity to the detriment of the general surgeon. I am, however, thoroughly satisfied that any intelligent person who believes in the efficacy of this mode of treatment, and is anxious to put it to the test, will not have much difficulty in acquiring the requisite amount of knowledge.

My attention was first directed to the subject of urethral electrolysis by the writings of Dr. Robert Newman, of New York, and again by a paper published by Dr. Steavenson and Mr. Bruce Clarke, of London. But what finally riveted my attention on the subject, and determined me in giving it a trial, was a conversation which I had with my cousin, Dr. C. H. Haines, of Auckland, some three years ago when he visited this country, in the course of which he asked me if I had ever given a trial to electrolysis in the treatment of gleet or stricture, and he mentioned in particular a case of gleet of two years' standing which he had succeeded in curing by two applications, after all the ordinary methods of treatment had failed. He said that it occurred in a naval surgeon who had been stationed in New Zealand. By one of those strange coincidences which serve to illustrate how small a world it is in which we dwell, this very surgeon appeared in my consulting-room a few weeks after this conversation took place, to consult me on another matter, and mentioned incidentally that he had been most successfully treated for a gleet of two years' standing by Dr. Haines, of Auckland.

The mode of treatment I adopt is nearly similar to that advocated by the authors I have referred to—any points of difference will be indicated as I proceed—but as some of my hearers may not be familiar with the usual technique, I shall briefly indicate the most essential points which require attention—

1. A suitable galvanic battery must be employed, the current from which will be fairly constant.

2. A galvanometer graduated in milliampères is absolutely necessary so as to indicate the exact strength of the current.

3. I think it is advisable to introduce a water Rheostat into the circuit, so that the current can be increased or diminished in a very gradual manner.

4. Insulated electrodes terminating in a conical or bulbous plated metallic knob, and of suitable sizes are required, both straight and curved. Those I first employed I obtained from Messrs. Coxeter & Son; they were straight, but could be readily curved if so desired, but not graduated. I, however, got this firm to make me some graduated in half inches, so that the exact distance of the part operated upon from the meatus may be readily known. I also employ stiffened electrodes, with the usual curve of a silver catheter, for making applications to the curved portion of the canal. These are provided with flattened handles, one side of which is rough, so that there may be no doubt as to the direction of the point of the instrument when introduced.

5. An electrode of large surface is required for external application.

The *modus operandi* is as follows:—The large external electrode having been thoroughly moistened with hot water is applied above the pubis or to the lumbar region. I prefer the latter situation, as the pressure of the patient's body upon it maintains a steady contact—a condition not to be calculated on when applied to the anterior abdominal walls owing to the respiratory movements—unless, indeed, a heavy one be employed, such as Apostoli's clay electrode—the weight of which causes it to exert a pretty uniform pressure. This external electrode must in all cases, where we employ urethral electrolysis, be connected with the positive end of the battery.

The urethral electrode being invariably made the negative pole, is passed down to the affected portion of the canal, and held there steadily; the current is carefully turned on to a strength of from two to ten milliampères—five milliampères being the usual strength employed. A gentle pressure is made on the urethral electrode, so as to cause it to glide through the stricture or over the inflamed surface—it must

be moved very slowly, and very little force should be employed, as the instrument will in most cases advance with very slight assistance. When the affected region has been passed, the current may be gradually diminished, and finally shut off before the instrument is withdrawn, or, if so desired, it may be very gradually withdrawn with the current still on, so as to make a second application to the diseased surface. The entire sitting may occupy from three to five minutes or considerably longer in exceptional cases. When dealing with granular urethritis it is best to employ an electrode somewhat smaller than will be sufficient to distend the urethra at the inflamed spot. On the other hand, when operating on a stricture we select one a size or two larger than the calibre of the stricture itself. After the current has been continued for some minutes it will with the use of very gentle pressure pass through an ordinary stricture, provided it is not too extensive or unusually indurated. Though a current of eight or ten milliamperes may be used, I am personally of opinion that weaker currents of three to five milliamperes continued for a longer time at each sitting will give equally satisfactory results, and with less distress to the patient. It has been stated by some authorities that the application of urethral electrolysis in the manner I have described is perfectly painless. This is not true of all cases. I have found that the application of a current of even two milliamperes to an irritable stricture, or a very sensitive patch of granular urethritis, sometimes gives rise to quite an unbearable pain, and in these cases I employ either an injection of five per cent. solution of cocaine when dealing with the spongy portion of the urethra, or pass in a soluble bougie containing one or two grains of cocaine, which can be slowly pushed down the urethra by means of the electrode I am about to employ.

When an application has been made it is often followed by an intense desire to urinate, which, if restrained by the

patient, will pass off in a few minutes. I am of opinion that it is better for the patient to restrain this desire when possible, and also to avoid micturating as long as possible after each *séance*, as the passage of urine on the first occasion after each application is usually attended with some amount of scalding, which becomes less on each subsequent occasion, and is much diminished by the internal use of alkalies and diluent drinks.

If so desired the urethra may be washed out with a warm, saturated solution of boric acid previous to making an application; but this is unnecessary, as the electrolytic action renders the application aseptic. I refrain from giving further details of the methods employed, for which I must refer you to other sources. However, I should mention that an interval of from five to ten days should elapse between successive applications, a week being the usual period.

In dealing with a case of stricture it will be found that, after passing an electrode of any particular size through the stricture at one sitting, when the patient is examined in a week afterwards, not alone will the instrument employed on the previous occasion pass with ease, but the stricture will usually admit one a size or two larger without the employment of force. Thus it will be seen that the amount of benefit resulting from the application is not merely the result of dilatation, as has been stated by the opponents of the electrolytic treatment.

I shall now proceed to relate to you some of the cases in which I have employed this treatment, in as brief a manner as will be consistent with a clear understanding of their nature and the results attained.

CASE I.—*Gleet; Slight Stricture at Bulb, with Patch of Granular Urethritis, two inches from Meatus. Successfully treated by Electrolysis.*—R. B.; history of gonorrhœa, with epididymitis; now has gleet of 18 months standing.

Examination.—Nov. 18th, 1889.—Passed No. 22 F bulbous instrument; this was arrested 6 inches from meatus. Then passed 19 F. graduated bougie; this was grasped tightly when passing through bulbous portion of urethra.

Treatment.—Nov. 20th.—Recommended electrolysis; applied No. 18 electrode to bulbous urethra for five minutes—strength of current, 5 milliamperes; also applied No. 27 to a tender patch in the anterior part of urethra for five minutes. These applications gave rise to no pain, but produced a hot sensation, which the patient had no difficulty in bearing. There was no subsequent pain or uneasiness, except on the first two or three occasions during micturition, when some scalding was experienced.

Nov. 27th.—Passed No. 20 F without difficulty; applied No. 22 electrode, 5 milliamperes. This passed through the constriction in three minutes. Then applied No. 24 electrode; this also passed through in three minutes.

Dec. 4th.—No. 24 bougie passed with slight resistance. Applied No. 24 electrode, with a current strength of 5 milliamperes; this was worked very slowly backwards and forwards at the seat of constriction for five minutes. I then introduced No. 27 electrode, this was arrested two inches from meatus; a current of 5 milliamperes was applied, and it passed through in three minutes. It was worked slowly backwards and forwards for seven minutes.

The patient was examined ten days afterwards, when No. 27 bougie passed through the entire urethra without any unusual resistance. The patient was subsequently examined on a few occasions at intervals of about a month, but no return of the tenderness or constrictions had occurred, and the patient has, to my knowledge, remained free to the present time from any return of his former troubles.

So far as the gleet was concerned, the amount of discharge underwent some increase in three or four days after the first and second applications—not after the third—but steadily diminished after a few days, and had ceased at the time of the next examination, and did not subsequently re-appear.

CASE II.—*Deep-seated Multiple Stricture with Perineal Fistula and exceedingly Irritable Urethra.*—G. B., recommended to me by the late Surgeon Corley, of Dublin, consulted me 3rd May, 1889.

There was a history of aggravated stricture complicated with extravasation of urine, a perineal fistula which still existed at the

time of examination, and through which a considerable quantity of urine escaped during micturition. There was also some œdematous swelling of the scrotum, and some purulent discharge from the urethra. The urethra was excessively tender and irritable, the most gentle passage of an instrument being attended with great pain. I was informed by Mr. Corley that he had a few months previously performed internal urethrotomy, and divided some exceedingly tough strictures in the deep urethra with a Maissoneuve's instrument.

At the time I first saw the patient he was able to pass a No. 7, and sometimes a No. 8, English-sized, soft rubber catheter, with which he drew off urine a few times daily, but not on all occasions. He had been using these instruments since recovery from the operation, but had not increased the size he employed, though directed to do so by Mr. Corley. I strongly urged the necessity of employing graduated bougies of increasing calibre to dilate the urethra, but after about a week's trial, finding that there was no progress being made, while the urethra remained intensely irritable and unyielding, and as it was impossible for the patient to lie by for any further operative or other treatment, I suggested the employment of electrolysis, with the object both of allaying the irritability and increasing the calibre of the stricture. Having obtained Mr. Corley's consent to this course, I commenced the electrolytic treatment on May 19th. The employment of even the feeblest current gave rise to so much pain that I injected a 4 per cent. solution of cocaine before proceeding with the application. I then employed a current of 3 milliampères. The second application was made on May 26th. The urethra was much less irritable; cocaine was again employed, and a current of 5 milliampères was applied. The third application was made on June 16th, and this time the patient was able to pass a No. 8 English catheter with ease, and the irritability of his stricture was so far reduced that the patient was able to bear the application without the aid of cocaine. After this application he was fairly well for four or five days, then got pain during micturition, and a swelling formed in the perinæum in front of the fistula. This all subsided in a few days, and the patient was able to pass a soft No. 8 without pain, but could not pass a gum-silk bougie of the same size. When seen by me on June 23rd the urethral discharge had greatly diminished—some urine still passes through the fistula, and the scrotum and penis are slightly œdematous. I passed a No. 17 F. bougie, which was

tightly held by the stricture. I directed him to use a soft instrument to draw off the water for the present, but postponed making any application. On July 22nd the patient expressed himself as feeling decidedly better; the swelling had greatly subsided, and the irritability of the urethra had practically disappeared. I again applied electrolysis.

Unfortunately in about a week afterwards the patient was obliged to leave Cork, so that the treatment had to be abandoned. He wrote to me a few weeks afterwards stating that his condition was decidedly improved, and expressing his regret that the treatment could not be continued.

In June, 1890, he returned to Cork, and asked me to make some further applications. Accordingly, on June 7th I again employed electrolysis. This time I succeeded in getting a No. 18 electrode behind the posterior constriction, and made the application while slowly withdrawing the electrode. On June 14th I used Nos. 18 and 20 in succession. On June 21st I again employed No. 18 and 20. On July 3rd I used No. 20 electrode for ten minutes. On July 11th I applied Nos. 20 and 22.

The patient had again to leave Cork, so that the treatment came to an end. But, as indicated by the records I have given, was benefited to the following extent:—

1. The irritability of his urethra had completely disappeared.
2. The calibre of his stricture had increased 15 mm. to 22 mm.
3. He was able to pass a good stream of urine without pain.
4. The fistula was much reduced in size, and permitted only a small quantity of urine to escape.

I am unable to give details of the subsequent history of this case, but I have reason to know that from carelessness about the use of instruments, and from occasional indulgence in free-living, the patient relapsed into his former condition, and was at least twice unsuccessfully operated upon for the cure of the fistula. I think it quite possible that a more successful result might have been attained by a continuation of the electrolytic treatment.

CASE III.—Impassable Traumatic Stricture of Spongy Urethra, treated unsuccessfully by Electrolysis, subsequently by Operation with successful Result.—M. D., farmer, aged thirty-one, received injury to perinæum early in Dec., 1889, by falling from the top of a load of corn, astride the heel of the shaft—the entire force of the fall being brought to a check by the impact on the perinæum—followed

by profuse urethral hæmorrhage. Had no treatment, but went to bed for a week, during which time there was some swelling at the scrotum and hypogastric region. There was also hæmorrhage at the commencement and after micturition, which act was accompanied with great pain. The dysuria became more and more difficult, and greater force was required to expel the stream, which steadily underwent diminution until it became a mere dribble. The symptoms became more and more aggravated, and he became so alarmed that he sought medical advice on April 14th, 1890. He was recommended to come to the North Infirmary, where he consulted me on April 17th.

On examination I found a stricture commencing $4\frac{1}{2}$ inches from the meatus, which was quite impassable to even the smallest instruments. He was made to pass water, which came away in drops, there being no continuous stream.

I recommended the patient to come into hospital, and submit to operative treatment. His objection to this course was so great that I determined to give a fair trial to electrolysis, as I believed the case was one which would answer as a severe test. The patient readily consented, and the electrolytic treatment was commenced on April 18th. On this occasion a medium-sized electrode was passed down to the face of the stricture; a current of 5 milliampères was used for twenty-five minutes, but without any appreciable advance. At the patient's next visit he expressed himself as being greatly relieved—the frequency of micturition being reduced to normal, while the size of the stream was decidedly greater and less force was used during its expulsion. Electrolysis was employed on five subsequent occasions, the current being increased first to 7, and afterwards to 10, milliampères. On each of these occasions two electrodes were employed—one of medium, the other of small size—each being kept steadily pressed against the stricture, but at the conclusion of the sixth *séance* the amount of progress that had been attained, so far as the introduction of the electrode was concerned, did not amount to one-eighth of an inch, while the stricture still remained impassable to the finest bougies, so I strongly urged the advisability of operation, to which the patient readily consented, as he fully recognised the gravity of his case.

He was admitted to the North Infirmary, where I performed the following operation, which I shall describe in detail, as it differs in some respects from the usual methods of procedure in such cases.

The patient was brought fully under the influence of chloroform; the urethra was first injected with a saturated solution of boric acid, then with carbolic oil, and I carefully endeavoured to pass a fine filiform bougie through the stricture, but without success. A metallic staff was passed down to the seat of stricture so as to mark its situation; I then deliberately cut down by a median incision through the perinæum, and laid open the urethra behind the stricture. I then endeavoured to pass a fine bougie from the perineal wound in a forward direction through the stricture, which I accomplished successfully. To the anterior extremity of this bougie I attached the end of the filiform bougie of a Tevan's urethrotome by means of silk, and drew it forcibly backwards through the stricture by pulling on the posterior end of the instrument that had been introduced from the perinæum. The metallic portion of the urethrotome was then attached to its guide, and by drawing on the latter from the perinæum the instrument was conducted successfully through the stricture, considerable force being necessary to accomplish this end. A very tough stricture, about half an inch in length, was then divided from before backwards along the roof of the urethra, after which I introduced a Thompson's urethrotome through the meatus until its bulbous end appeared in the perinæal wound, and divided the strictured portion of the urethra on its inferior aspect from behind forwards. This being accomplished, I passed a No. 18 English curved metallic dilator (such as I now show you) through the entire length of the canal. When this was withdrawn I introduced a large gum-elastic catheter from the perinæal wound into the bladder. This was tied in for some days, the urine being drawn off at intervals, so that none of it was allowed to pass through the previously strictured portion of the canal. After the divided urethra had been thus permitted to rest for four or five days, a large catheter was passed from the meatus, and tied in until the perinæal wound was almost healed. After this the instrument was withdrawn, but was passed each time by the patient that he required to draw off water. Metallic dilators were also occasionally introduced.

The perinæal wound quickly cicatrised, and on the patient leaving hospital I was able to pass a No. 18 English dilator without difficulty—this being the largest size that the meatus would admit. The patient was given an olivary gum-silk bougie, No. 26 F, with instructions to pass it at suitable intervals. He

was subsequently examined on a few occasions by me, when I found his condition remained satisfactory.

The operation I have described serves to illustrate the combination of internal urethrotomy with perineal drainage through an external urethrotomy performed behind the seat of stricture, a procedure which I believe was first advocated by my friend, Mr. Reginald Harrison, and which I had previously employed in a few suitable cases with equally satisfactory results.

The deductions to be made from the use of electrolysis in this case I shall refer to later on.

CASE IV.—Neglected Multiple Stricture of Ten Years' Standing Treated Successfully by Electrolysis.—M. G., aged thirty-two, consulted me June 17th, 1890. Got stricture ten years ago following gleet, which was not treated until four years after; then got it dilated. Had since occasionally passed bougies, but neglected himself of late, so that now he is only able to pass No. 2 English bougie. On examination I found a lunated stricture on the floor of the urethra, three-quarters of an inch from meatus. This allowed No. 20 F bulbous instrument to pass, but it caught during withdrawal. Another of 8 mm. calibre existed two inches from meatus; a third of 5 mm. extended from a point five inches from the meatus as far the membranous urethra.

The lunated stricture near meatus was first divided, and electrolysis was applied to the middle and posterior strictures on a few occasions; but, finding the middle stricture interfered somewhat with the treatment of the posterior one, I divided it from behind forwards with a Thompson's urethrotome, and as soon as it was healed I continued the electrolytic treatment of the posterior, to which the electrolysis was applied on six occasions at variable intervals, with the result that, after the 6th application, a No. 22 F bougie could be passed without difficulty into the bladder. The patient being then quite free from all vesical irritation, and able to pass a good-sized stream, was so satisfied with his condition that the treatment was discontinued. From the unyielding nature of the stricture in this case as well as the previous history of his progress under dilatation, I am satisfied that an equally satisfactory result could not have been attained by the process of general dilatation.

CASE V.—*Gleet of Eighteen Months' Standing, due to Granular Urethritis, Cured by Electrolysis.*—H. R., aged twenty-eight, consulted me on January 24th, 1891. Got gonorrhœa in June, 1889, for which he was treated in Dublin. Has never been quite free from slight discharge or moisture; also complains of an uneasy sensation in the perinæum; wished to be examined as he was contemplating marriage.

I passed a No. 23 F. olivary bougie without encountering any obstruction; but it gave rise to a disagreeable sensation in the perinæum. A bulbous sound indicated the presence of a tender patch and some irregularity at the region of the bulb. Examination with Leiter's urethral electroscope showed the presence of a congested granular patch, partially covered with muco-purulent discharge at the bulbous urethra. Electrolysis was recommended and applied on three occasions at intervals of a week, a current of 5 milliampères being used. The first application was attended with considerable pain, the second with very little, and the third was painless, being accompanied with only the usual hot sensation due to the galvanic current. On March 21st the patient was practically well, being free from all uneasiness and discharge; but at his own request I made another application. He returned on April 4th, expressing himself as being thoroughly cured. He got married shortly afterwards, and has remained quite well since.

CASE VI.—*Sexual Weakness and Imperfect Micturition, resulting from repeated Gonorrhœa, treated successfully by Electrolysis.*—W. C., aged thirty-two, draper, has had gonorrhœa on several occasions, also was told he had a slight stricture, which was treated by gradual dilatation. At present complains of dribbling of urine at end of micturition, and imperfect sexual power, with too early orgasm; there is no gleet. Examination did not reveal the existence of any stricture or granular patches, but the membranous and prostatic portions of the urethra were in a hyperæsthetic condition, and the passage of an instrument through this portion of the canal immediately produced an erection. I recommended electrical treatment, and applied the negative pole with a current of 5 milliampères to the affected portion of the canal on four occasions, at intervals of from seven to ten days. The result was entirely satisfactory; the over-sensitive condition of the canal completely subsided, also the tendency to priapism,

while the natural functions were restored to the entire satisfaction of the patient.

CASE VII.—*Case of Gleet following on Gonorrhœa Successfully Treated by Electrolysis.*—J. H., aged twenty-five, accountant, had acute gonorrhœa, complicated with prostatic abscess and epididymitis six months ago; still suffers from urethral discharge, which he has failed to cure by the careful use of various injections, and the internal administration of the most approved urogenital remedies. Examination revealed the presence of granular urethritis affecting the bulbous and anterior half of the membranous urethra.

I submitted him to the electrolytic treatment, three applications being made at intervals of about a week, the first being a current of 3 milliampères, the second and third, 5 milliampères each. About six days after the last application all sign of discharge ceased. There has been no return, and he is now perfectly well.

I could multiply cases of a somewhat similar character to this; but I think I have given a sufficient number to illustrate what I desire to bring under your notice.

I shall now proceed to notice the deductions which may be made from the foregoing cases, which, though limited in number, are sufficiently diverse in their nature to afford some useful indications as to what may or may not be expected from the employment of electrolysis in the manner I have indicated. I shall deal with the cases in the order which appears most suitable for this purpose.

Cases I., V., and VII. are typical examples of the class of cases which, in my opinion, can be rapidly and permanently cured by the electrolytic treatment—that is where we have chronic gleet due to the existence of granular urethritis in the deep urethra which may or may not be associated with a slight amount of stricture. These are the cases in which Mr. Berkeley Hill, in his lectures delivered at the Royal College of Surgeons, London, in 1889, on chronic urethritis, strongly advocates the local application of nitrate of silver solution applied through an endoscope, which treatment has

proved most successful in his hands. I am, however, of opinion that the electrolytic possesses distinct advantages over such treatment—First, because it is much less painful; second, the applications are not followed by fresh inflammation, such as often follows the use of nitrate of silver; and, third, the number of sittings required to produce a cure is fewer than with the caustic treatment.

Case II. serves to illustrate how, even under most unfavourable conditions, considerable benefit may be obtained by the use of electrolysis. The most notable feature in this case was the rapid manner in which extreme irritability of the urethra was relieved by a few applications, and in addition a very tough and unyielding stricture was considerably enlarged in calibre, though it resisted dilatation by the passage of bougies in the ordinary manner. The patient was himself very strongly of opinion that, if he had been permitted to remain under my care for a sufficient time, his stricture would have been completely cured by a continuation of the treatment—that in all probability a cure of the fistula would also have resulted—an opinion with which I see no cause to disagree.

Case III.—A consideration of the course of electrolytic treatment in this case shows that we need not hope for much benefit from its use in the ordinary way in cases of impassable traumatic strictures. Where, however, an electrode can be made to enter the strictured canal, a more favourable result might be anticipated; but I have no personal experience of such cases. Perhaps, if the method advocated by Dr. J. A. Fort was employed in the case I have related, a more satisfactory result might have been obtained. “In this method the active portion of the electrode consists in a linear exposure of metal at or near the point of the sound. By the use of this instrument a narrow furrow can be ploughed through the constricted portion of the canal and afterward

dilatation, either with or without electricity, practised. Dr. Fort also uses this method for all ordinary strictures, and reports satisfactory results with it."

Case IV. is an example of a case of neglected and indurated stricture which yielded in a very satisfactory manner to electrolysis. In this case gradual dilatation had previously failed, and had the electrolytic treatment not been employed, the patient would soon be compelled to submit to some radical form of treatment, such as internal urethrotomy.

Case VI.—I have introduced this case to your notice to illustrate how a very distressing and common ailment—namely, sexual weakness depending upon a hyperæsthetic condition of the membrano-prostatic urethra may be relieved by the suitable employment of electrolysis. I have experienced equally satisfactory results in some other cases of a similar nature, but refrain from occupying your time by relating their details.

To sum up, I think we may fairly conclude that electrolysis is capable of yielding very satisfactory results—

(1) In cases of gleet due to granular urethritis.

(2) In cases of simple stricture, such as are ordinarily submitted to the method of gradual dilatation—and it is claimed by those best qualified to speak on the subject, that the cures effected by its use are permanent, a result which certainly cannot be claimed for the ordinary surgical methods of treatment.

(3) That even very rigid and old-standing strictures can be greatly relieved, if not absolutely cured by its use.

(4) That it has remarkable power in relieving irritability of the urethra, and removing the distressing desire for frequent micturition which so usually accompanies it.

(5) That it may be of considerable use in dealing with cases of supposed or real loss of sexual power, such as are too well known to every surgeon of experience, and which

constitute some of the most distressing cases with which we come in contact.

I have refrained in this paper from referring to some other affections of the genito-urinary organs, in which electrolysis has been advocated. I have also purposely abstained from entering into any theoretical discussion as to the manner in which electrolysis brings about the results claimed for it. I must, however, point out that this operation, with the strength of current employed, is not in the nature of a cauterisation—it is electrolysis pure and simple. There is no elevation of temperature in the electrode, and no charring of tissue. The pathological tissue is slowly dissolved by the electrolytic action of the current, which probably also stimulates the absorption of infiltrated products.

In conclusion, I think I may claim to have shown that electrolysis is a remedy very potent for good in suitably selected cases, and fully worthy of our best consideration.

SIR WILLIAM STOKES congratulated the author on the interest and importance of his communication. He could not, however, endorse what had fallen from the author in reference to the permanence of the cure of stricture by electrolysis. In fact, in Sir W. Stokes' opinion, after no method of treatment can it be honestly said that a cure has been effected, and in his experience a return was just as likely to occur as after the treatment by internal or external division or gradual dilatation. In the treatment of gleet, Sir W. Stokes thought that Dr. Pearson's experience should stimulate surgeons to give it a full trial.

MR. THOMSON said he had used the method described by Dr. Pearson in gleet with success. But his experience in stricture was not satisfactory, and he had not pursued the treatment. He was acquainted with Dr. Newman's claims, and the work that had been done by Dr. Steavenson. But if the treatment was so successful as was claimed, was it not curious that all surgeons the world over did not adopt it. The question was really—Was stricture of the urethra curable? He (Mr. Thomson) had never seen a case

where, no matter what method was adopted, he was able to say to a patient: You are cured; that is, that the person could go on without the periodical use of a bougie. He had never met anyone who claimed such a result. Was it attainable by electrolysis? If it was, they ought all to adopt this plan, but until such a claim could be substantiated he did not think that electrolysis had any advantage over some of the simpler methods.

MR. SWAN said that, for his part, he did not believe in the permanent cure of stricture by electrolysis, no more than he believed in the same result by Holt's method, by gradual dilatation, or by any form of urethrotomy. He remembered, when Holt's method was in fashion, it was thought that stricture was at last conquered. Many persons, he believed, were Holted (if he might use the term) who never had stricture. Only last month he had used Maisonneuve's instrument on a gentleman, the possessor of a tight stricture, who had been cured for a time by electrolysis.

MR. M^CARDLE said the only experience he had of this treatment for stricture was that he had been obliged to use Maisonneuve very frequently on cases of so-called cures by electricity. As to the good effect of the application of the electric current to patches of granular urethritis and small patches of ulceration, there can be no doubt, and if large leaden electrodes, carefully guarded, were used, the healing should be more rapid.

MR. TOBIN said the question of the permanent cure of stricture by electrolysis resolves itself into this—Can electrolysis cause the disappearance of cicatricial tissue? His opinion was that it cannot. However, on hearing the excellent paper just read, he should again try; and the case he should select would be one of stricture at the orifice of the urethra. If it then succeeds, if it causes the disappearance of a stricture that he could see, then he should believe in it, and extend its application to all cases that he met with.

ABSCCESS OR ANEURYSM.

By J. K. BARTON, F.R.C.S.;

Senior Surgeon to the Adelaide Hospital ; late President of the
Royal College of Surgeons, Ireland.

[Read in the Section of Surgery, April 8, 1892.]

THE diagnosis between an aneurysm and a deep-seated abscess in the same region has always been a subject of great interest to the practical surgeon, and much as we may pride ourselves upon the modern advance of our art, cases still occur in which this old question arises again, or causes great differences of opinion. In the case I am about to relate, so nicely were the signs balanced *for* aneurysm and *against* abscess, or *for* abscess and *against* aneurysm, that more than one-half the staff of the hospital stood boldly for the diagnosis, which finally proved erroneous—by that of aneurysm—for it was an abscess.

In the case of a popliteal aneurysm, an abscess may very closely simulate its signs, when the purulent collection is deep, bound down by the deep fascia. Under these circumstances, so much pressure may be made by the confined matter as to retard the circulation in the artery, at least to some extent, and thereby simulate one of the most reliable signs of aneurysm. For I take it that the most certain diagnostic signs of aneurysm are—first, the effect produced on the circulation beyond, and, secondly, the *distensile* pulsation of the tumour. While those of abscess, on the other hand are—*first*, deep fluctuation ; *second*, superficial œdema. In the extremities, either upper or lower, these will usually guide aright, but in the thorax and abdomen they no longer equally apply.

The late Mr. Dease's case was a striking illustration. It was a case of ileo-femoral aneurysm in which suppuration had taken place. On viewing the tumour, Mr. Dease gave his opinion that it was an abscess, and, although his colleagues held an opposite view, he plunged a bistoury into it, purulent matter flowed out, and Dease looked up with a triumphant expression; but in a moment this was followed by a fearful rush of arterial blood, which quickly terminated the patient's life.

I will now relate the particulars of the case which has led me to make these remarks.

CASE.—Richard Latimer, aged eighteen years, a very pale young man, was admitted in January 19, 1891, into the Adelaide Hospital under Dr. Beatty's care. Since the previous April—that is, eight months before his admission—the patient had been suffering from pain in the left side, extending down both arm and leg of that side, but most severe in the side of his chest. This pain got worse when he lay down in bed, so much so that he used to arch his back forwards to obtain some relief. He noticed a swelling in the upper part of his left chest wall for the first time on Sunday, the 1st of December, 1890. He was going to bed when this swelling was noticed. Next morning he went to the neighbouring dispensary, when Dr. Falkner saw him, and, he says, gave him a plaster to put on the tumour. He was a printer by trade. He now abandoned his employment—rest being ordered—and since has had much less pain. When questioned as to any injury having been inflicted on his chest, he remembered that in March, 1890, just a month before the pain began, a very heavy iron bar had fallen against the front of his chest.

Dr. Falkner discovered pulsation in the tumour, and advised him to go to the hospital. Upon examination, a pulsating tumour, about the size of half an orange, was found projecting from the chest wall, at the upper part of the cardiac region. The pulsation is systolic, and the second sound is accentuated over the tumour, but except this there is no murmur. No râles are heard through the lungs, but there is very feeble breathing all through the left lung and over the posterior part of it. There is dullness on percussion. The side is not all retracted. The pulse is the same on

both sides, and seems of equal volume. The sphygmographic tracings of each had the same characters; there was a little marked dirotic curve.

Feb. 15. The tumour has greatly increased in size during the past month. Its measurements are now as follows:—Vertical, $4\frac{1}{4}$ inches; transverse, $3\frac{3}{4}$ inches. There is a *strong pulsation* over it, and to the fingers placed on either side of the tumour this pulsation has a very expansile or distensile feeling. The next important point regarding this tumour is that it is *very tender* to touch, the patient wincing or even crying out when it is pressed or handled, and further *fluctuation* can be distinctly felt in it. The left side of the chest continues dull on percussion, except just along the spine, where the note is clear; the chest expands very sluggishly on the left side; breath sounds under left clavicle are feeble and almost inaudible laterally and posteriorly. Temperature normal.

Feb. 16. A consultation was held and the diagnosis as between abscess and aneurysm discussed.

In favour of the latter, or aneurysm, were:—First, the strong pulsation; second, its distensile character; third, the clearness of the cardiac sounds; fourth, the accentuation of the second sound.

For abscess were:—First, the age and strumous appearance of the patient; second, the position of the tumour; third, the great tenderness; fourth, local œdema.

In view of these signs, it was determined to puncture the tumour with a hypodermic needle, and thus demonstrate its contents. This I accordingly did then and there, and slowly drew, into the glass cylinder, pus. At once a free opening was decided upon, and the patient was carried down to the operation theatre, there placed fully under the influence of ether, and with the aid of my colleagues I proceeded to lay open this pulsating, purulent collection. Although I gave my advice strongly for this mode of treatment, yet I confess I never felt more nervous at any operation. While no doubt could now be entertained that we had to do with an *abscess*, yet it looked so like an aneurysm that I felt very queer, and when, having made a free incision, a dark greenish matter came freely out, my assistant drew back, muttering "*laminated fibrine*." But no, it was dark sanguine pus. I washed out the cavity with a boric-acid solution very thoroughly, and then introduced my forefinger to ascertain if possible the extent of the cavity. I found a cavity about the size of an orange, but with an opening at its left posterior side, which may have communicated either with the

pericardium or left pleura. I made no further investigation on this point. But having washed the cavity again, I drew the edges of the sac up to the skin, and secured complete drainage by stitching the opening in the sac to the skin, at the lower end of the wound. It could not be drawn together by deep or superficial sutures.

He improved very markedly after the operation, and gained in weight. His temperature varied very much, sometimes as high as 101° , more frequently normal. The wound continued to discharge a weak pus, but not in any great quantity. The left lung, which had been dull on percussion, became resonant, and respiratory murmur was again audible through it.

He went to the Convalescent Home when the summer came, and for a little time seemed to be gaining health. A cheesy, strumous discharge continued to come from the wound. Small strumous suppurations took place in several places, and the urine was found to be very albuminous. He died during the summer.

Mr. Going conducted the *post-mortem* examination, and reports as follows:—On laying open the chest the operation incision was found to lead to the cavity of the left pleura; there was no opening into the pericardium. The pleural cavity was almost obliterated by dense adhesions between the visceral and parietal layers. The sinus led into a space on the anterior surface of the left lung, surrounded on all sides by firm adhesions. This space contained a quantity of curdy, purulent matter. The back of the left lung was also bound down by adhesions, but they were comparatively easily broken down. Under the parietal pleura were found, here and there, masses about the size of a hazel-nut, each of a cheesy, strumous matter.

Over the whole of the right lung, and more especially in front, were very distinct adhesions; so much so that the lung tissue had to be cut away to open the chest.

On opening the pericardium a small quantity of sero-sanguineous fluid escaped; the pericardium was covered with a thick honey-comb-looking substance which formed a tough membrane lining to it. The heart, when its cavities were opened, was found free of any valvular disease; the muscular tissue seemed atrophied. Extensive amyloid degeneration was taking place in both kidneys, liver, and spleen.

In order that the interesting lesson in diagnosis taught by this case may remain clearly in all our minds, I would con-

clude the record of the case by reminding my hearers that the true nature of the case appeared when full weight was given to the age, temperament, and constitution of the patient. Looking at the young man, and considering his constitutional tendency, it became evidently much more likely that he should suffer from abscess than from aneurysm.

MR. KENDAL FRANKS corroborated Mr. Barton's account of the pulsating tumour, and of the difficulty of diagnosis. His own opinion was that it was a case of aneurysm, and the only doubt he had in the matter was that from the position of the tumour—namely, the upper portion of the cardiac region—it was difficult to imagine from what part of the aorta it could spring, unless it were an aneurysm of the heart. The distensible pulsation was very remarkable, and the weight of evidence seemed to him to be so strongly in favour of aneurysm, that he was surprised to find his colleague, Mr. Barton, equally certain that he had an abscess to deal with. But the result proved that Mr. Barton was right, and he had no hesitation in confessing that he was wrong.

MR. M'ARDLE related a case in which a tumour, the size of half an orange, projected over the junction of the second costal cartilage with the sternum. Pulsation was distinct and distensible; there was dulness over the upper third of the sternum; cough was distressing and persistent. In this case, after a severe fit of coughing pus was expectorated, and the tumour collapsed, thus rendering certain the diagnosis of tubercular abscess already made. Free incision and removal of the cartilage of the second rib allowed free access to a cavity behind the sternum. This he found contained tubercular matter, which he removed with a Volkmann's scoop, which during operation was constantly tilted upwards by the aorta. Plugging the cavity with iodoform gauze completed the operation, and the case progressed favourably. This case illustrates the value of Mr. Barton's communication.

SIR WILLIAM STOKES fully concurred in what the Chairman said as to the great clinical importance of the remarkable case Mr. Barton had detailed, and which illustrated so well the great difficulties that at times surround the diagnosis of such cases. Mr. Barton had alluded to the historic case which was connected with

Mr. Dease. The facts were not quite accurately stated by Mr. Barton. The case occurred in the old Meath Hospital, now the site of the Coombe Hospital; Dr. Whitley Stokes, S.F.T.C.D., then one of the physicians to the hospital, was present, and related the particulars of the occurrence to his son, Dr. William Stokes, who succeeded him. The case was a mixed one of aneurysm and abscess, the aneurysm being ilio-femoral, around the sac of which an abscess formed. The abscess was freely opened, and the hæmorrhage being uncontrollable, the patient sank. Mr. Dease exhibited no emotion, visited the patients in all his wards, and then returned home; he then entered his study, and with the same instrument with which he had operated on the abscess, he opened his own femoral artery. This tragical event was a proof, if one were wanting, of how deeply at times men may feel without giving any expression to it.

MR. MYLES, when discussing the question of diagnosis, gave details of a case which occurred during his student days, under the care of the late Dr. B. M'Dowel, and in which a patient suffering from innominate aneurysm was cured by medicinal treatment, and who afterwards developed a fluctuating swelling in the neighbourhood of his right sterno-clavicular joint—pulsating, growing rapidly, painful, and tender. Everyone who had followed the case believed this swelling to be a recurrence of the aneurysm; Dr. M'Dowel, however, not only disagreed with the general opinion, but declared his intention of cutting into the swelling. He did so to the horror of those standing near, and fortunately verified his diagnosis. Mr. Myles asked if the temperature of Mr. Barton's case were noted, and on what hypothesis other than that of an existing empyema the dulness on percussion and absence of breath-sounds in the left side were explained.

MR. BARTON replied.

SECTION OF OBSTETRICS.

DYSTOCIA DUE TO A CYST IN THE LIVER OF A FŒTUS.

By WM. S. BAGOT, M.D. UNIV. DUBL.; L.M., &c.;

Ex-Senior Assistant Physician to the Rotunda Hospital.

[Read in the Section of Obstetrics, Nov. 27, 1891.]

THE specimen which I have the honour of submitting to the notice of the Academy is, to my mind, one of extreme interest, not only from a pathological, but also from an obstetrical, point of view; for the records of cases where congenital cysts of any size have been found in the liver are very rare, and still rarer are records of such, or indeed of any tumour of the foetal liver causing dystocia.

The history of the case, from which this specimen was taken, is as follows:—

About 6 a.m. on May 12, 1890, word was brought to me, then Senior Assistant Physician to the Rotunda Hospital, that two of the intern students were in attendance on a woman in the Extern Maternity Department. It was her second pregnancy, her first having terminated normally at full time. She had now been in labour thirteen hours. The head of the child had been born immediately after the arrival of the students; but the birth of the body was delayed, and, though they had tried both expression and traction, they were unable to complete its delivery. I accordingly hurried to their assistance, and found, as they had reported, the head, somewhat smaller than normal, completely born, and the child apparently dead, the delay in the delivery having proved fatal.

The woman stated that she was about $8\frac{1}{2}$ months pregnant. Her body was covered with a papular eruption of syphilitic character. I could elicit no history of any specific sores having been on the

genitals. I then administered chloroform, and proceeded to palpate her abdomen. I found the uterus much larger, and more tense than is usual at full term, though some of the liquor amnii must have escaped on the rupture of the membranes, and on percussion a very distinct fluctuation thrill could be obtained.

The back of the child could with difficulty be mapped out, looking forward, and to the left in the first position. It at once occurred to my mind that I had to deal with a case of hydrops amnii, complicated by some abnormality of the foetal abdomen, such as ascites, or an ovarian tumour, &c. Accordingly, after a thorough disinfection, I passed my hand up posteriorly along the anterior surface of the child, and on reaching the uterine cavity, which still contained an abnormally large quantity of liquor amnii, I found, in accordance with my expectations, that the abdomen of the foetus was enormously distended and elastic. I now tried to let off as much of the liquor amnii as possible, but the attempt met with little success, for the great elastic belly of the foetus filled out the lower uterine segment, acting like a ball valve. I then perforated the child's abdomen close to the xiphoid cartilage by means of a Smellie's scissors, after which a great quantity of clear yellow fluid poured out, mixed with the liquor amnii, which now came away freely. The abdomen having collapsed, I easily delivered the child, a male, the placenta following almost immediately. After a great deal of trouble we succeeded in persuading the female relatives present to allow us to remove the child. The husband, luckily, was absent. On reaching the Rotunda Hospital I stitched up the hole made by the perforator, and by means of an aspirator filled the abdomen, from which all the fluid had drained away. I had to inject 60 ounces of fluid before it assumed its original degree of tension.

The following measurements were then taken:—Length of foetus, 45 cm.; girth at level of umbilicus, 59·75 cm.; distance from ensiform cartilage to pubes, 39·5 cm.

I then photographed it, unfortunately not very successfully, owing to haste, for the father having returned, and found that the child had been removed, had followed us and demanded its restoration. Finding it was impossible to pacify him, we hastily performed the autopsy. On opening the abdomen it was found that the chief abnormality existed in the liver. The rest of the viscera were normal, as far as could be made out from a hurried examination of them *in situ*, with the exception of the kidneys, which were

small. Accordingly we removed the liver and kidneys for further investigation, and restored the body to the relatives.

On examining the liver we found the right lobe comparatively normal, perhaps slightly small, and on the under-surface near the right edge a small lobe marked with parallel ridges. The gall bladder was absent, and the lobus quadratus consequently not marked off. Hepatic artery and vena portæ normal. Hepatic duct present, but rather small. It was not possible to trace a branch to the left lobe with any certainty, but it was probable that the left duct was torn from its connections on account of the haste with which the autopsy had to be performed. The duodenal end of the duct was not observed.

The whole of the left lobe was converted into a large simple cyst, capable of containing 48 ounces of fluid. It was smooth on its surface and marked by ramifications of vessels and ducts. The inner surface of the cyst wall was also smooth, except that it was covered by a whitish film, which could easily be stripped off. The fluid from the cyst, having all drained away at the time of perforation, could not be examined. Histologically the hepatic tissue was normal. The wall of the cyst was formed of two layers, about equal in thickness. One next the cavity of the cyst consisted of a rather dense fibrous connective tissue; and outside this, over the greater part of the cyst wall, a layer of equal thickness, consisting of hepatic tissue. Where this is absent, its place is taken by large vessels and ducts. Near the junction of the two layers numerous small ducts could be seen; but these in no case at all approach the cavity of the cyst, they open into the large ducts, mentioned above as accompanying the vessels. No epithelium could be demonstrated lining the interior of the cyst. The kidneys, though very small, were normal in structure.

Now, on referring to the literature of the subject, I can find records of only two cases, which appear to me to bear any points of resemblance to that which I have just described.

The first, reported by Dr. Witzel (*Centralblatt für Gynäkologie*, No. 24, 1880), where he records a case, which occurred in the obstetrical polyclinic of Prof. Gusserow. During the summer semestrium Dr. Witzel observed a woman, aged thirty-six years, who was in labour. It was

her second pregnancy. Some years previously she had borne a normal child at full term. The midwife not being able to determine the presentation, sent for his assistance. External examination showed that there existed a longitudinal lie of the child, and he could feel on the left side a rounded part; but above the symphysis pubis no hard tumour corresponding to the head could be found; accordingly he thought it was most probably a breech presentation. On examining *per vaginam* he found a soft part, and towards the left a pedunculated sac, bigger than a walnut. Behind this there was an opening, into which the tip of the finger could be put. On one side of this opening he could feel a resistant ridge, and behind this a resistance covered by soft parts. He consequently diagnosticated it as a second position of the breech. No progress was made in the labour during the next eighteen hours. After this, owing to some strong pains, the child was born so far that a malformed head was seen in the vulva. While the patient's husband was bringing word to Dr. Witzel, a neighbouring doctor was called in, who tried to effect delivery by pulling on the head till he lacerated the soft parts of the neck as deeply as the spine, and almost completely separated one arm. Dr. Witzel, on his arrival, seeing the malformation of the head, made further examination with the patient under chloroform. He could still feel the rounded part in the fundus of the uterus; it seemed to correspond to a second head. However, on examining *per vaginam*, and passing up his hand, he found that this was in reality the short legs lying close to one another, and he could feel the belly blown out like a balloon. He therefore decided to perforate it. Upon this being done, three litres of yellow fluid poured away, and the child was then easily delivered.

The examination of the child showed the following:--

Rudimentary external female genitals.

Thick short upper extremities, with six fingers on each hand, and rudiment of a seventh on the left.

Six toes on each foot; genu varum and pes varus of both extremities; hemicephalous, greater part of the skull being absent, presenting pedunculated membranous sac; palpebral fissures small; orbits small; no eyeballs; nose flat; complete absence of inter-maxillary bone; situs inversus viscerum totalis; cystic liver, the left lobe being, with the exception of a small portion of the anterior margin, completely converted into a cyst with irregular surface. This cyst communicated by an opening, admitting two fingers, with a smaller one in the right lobe; the other lobes were normal; the gall-bladder was empty. The cystic duct was solid, while the common duct, which opened into the cyst in the right lobe, was dilated to the thickness of one's thumb, and ended blindly towards the duodenum; but in the duodenum the usual papilla could be made out. There was a Meckel's diverticulum. Each kidney was the size of a man's fist, and cystic, with small cysts the size of a pea; the ureters were not dilated; the bladder was empty. There existed other malformations, which I need not delay to describe.

The second case, reported by Lomer (*Virchow's Archiv.*, 99), I think worthy of note from the fact that the foetus presented signs of congenital syphilis, though the cystic liver did not cause any obstacle to delivery.

Here there was obliteration of the cystic and right hepatic ducts. The gall-bladder was collapsed and obliterated; the left lobe of the liver was normal; the right lobe was rough, convoluted, like a brain, and degenerated, with cysts in it; a smooth cyst projected from the surface of the liver; it was the size of a cherry, and communicated with other cysts in this mass of scar tissue; the left duct, the hepatic artery, and the vena portæ were normal; the cysts contained a yellow fluid; the foetus was macerated, and its bones presented

syphilitic lesions. Lomer considered the malformation to be due to a primary developmental defect, or else to syphilitic perihepatitis.

Turning now to the obstetrical bearing of this case, which, though not of such general interest, is none the less of great importance, I find that the records of cases where tumours of the liver have acted as obstacles to delivery are extremely few. Winkel, in his "Text-Book of Obstetrics," one of the most recent and complete works on the subject, states that "the description of those tumours of the liver, which have been causes of delay in or obstacles to delivery, is somewhat defective." He mentions records of four cases—viz., (1) Haase (*N. Z. XI.*, 262); (2) Müller (*Hohl*, p. 286); (3) Nöggerath (*Deutsche Klinik*, 1854, No. 44; *Wochenschr. IV.*, 458); (4) Schlesinger (*Hohl*, p. 289). The first three, according to Winkel, "resolve themselves into either hepatic physconia, or a lymphatic tumour of the liver." In Nöggerath's case the liver was the seat of a congenital carcinoma, and weighed $2\frac{1}{4}$ lbs.; it measured 8·75 inches in width, 3 inches in thickness, and 6 inches in height. The liver in Müller's case, a lymphatic tumour, weighed 4 lbs. These four cases, together with Witzel's, which has been overlooked by Winkel, and the case which I now report, would amount to but five cases in which I have been able to find records of any obstacle being offered to delivery by a tumour of the liver. The head of the child presented in all the cases, with the exception of that reported by Haase. In it the fœtus presented obliquely, with the head towards the left, and the cord and left arm prolapsed. The liver in this case weighed one pound. The child was a girl, 19·5 inches long, and 9 lbs. in weight.

ANÆSTHETICS: A CLINICAL STUDY.

By DUDLEY W. BUXTON, M.D., B.S., LOND.;

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[Read in the Section of Obstetrics, Jan. 8, 1892.]

I CANNOT but feel greatly honoured and gratified that you have permitted me to address you upon this subject. My theme needs no apology, and whatever deficiencies you detect in the manner in which I may unfold its many sides to you will, I hope, be forgotten in the interest of the subject itself. Anæsthetics are, of course, a thing of old-world lore, but the knowledge of the systematic induction of anæsthesia by the formal use of chloroform, ether, nitrous oxide, and their congeners dates from less than half a century ago. What knowledge, therefore, we have upon this point is well within reach; consists, if I may be permitted to say so, of a large amount of undigested material dealing in the main with the physiology of anæsthesia as studied in the lower animals, and a certain number of records of deaths occurring during the use of one or another anæsthetic. To this catalogue we must add a good allowance of more or less acrimonious discussion between the partisans of one or another school of thought. In the early days of chloroform we met with pietists crying aloud in the wilderness of the lay press against the iniquity of giving parturient women chloroform, and so defeating the aims and mandates of a Providence which had ordained that in sorrow should woman bring forth children, and in these

later days we find argument is tempered by personality, while the tedium of science is relieved by wrangling. A careful study of this literature justifies me, I think, in saying that up to the present time no systematic effort has been made to treat the study of anæsthetics as a clinical research, and employ the same acumen, the same rigid methods, and unimpassioned words in obtaining records as those we should insist upon were we about to grapple with, say, tachycardia, Raynaud's or Thomsen's disease; or the action of strophanthus or ulexine upon the organism. It is to urge the necessity for such a clinical study that I am here to-night.

I admit readily enough, and I have no doubt I shall be reminded again and again in the course of the discussion, that many valuable, though brief, monographs dealing with the matter have from time to time appeared, but I submit, the general bearing of the profession with regard to anæsthetics has been one which cramped the study, lost its leading lines by running into side issues, and finally, dwarfed the significance of the whole matter by accepting the *dicta* of great men, instead of seeking the *facta* of every-day experience. Most men pass through two stages at least—the book-knowledge stage, in which, chrysalis-like, they accept a store of food supplied without initial labour; and secondly, a butterfly stage, when they discard mere book-lore, and test all knowledge by what they see at their patients' bedsides. So is it with anæsthetics; many of us have believed that all that was to be known had by this time been set forth in books, so that no further appeal to the experience of the eye, the hand and the mind was deemed necessary. So assured are many persons of the soundness of their theories that, discarding the usual caution of scientists, they tell us that this *quæstio vexata* is now and for ever settled, and that further research is, besides being a weariness of the flesh, futile, if not impertinent. But such, surely, are not the ways of science.

Let us say, *we believe* rather than *we know*; *we conclude* rather than *we have proved*.

The necessity for the clinical investigation into the action of anæsthetics seems to be necessary for many reasons. In the first place, we have already had the benefit of many most careful researches conducted upon the lower animals, with results which are at present, to say the least of them, quite irreconcilable, but which have been conducted so carefully, and by such able experimenters, that we are bound to give them full credit for their work, and might well be "happy with either were the other but away." Thus, in 1855 the Commission appointed by the Society of Emulation in Paris came to the conclusion that in chloroform poisoning the heart always survived the cessation of the respiration. This view was reversed by that taken by the Committee appointed by the Royal Medico-Chirurgical Society of London in 1864. The finding, again, of the Glasgow Committee of the British Medical Association was adverse to chloroform; it pointed out not only the liability of heart failure, but laid stress upon the dangerous fall of blood pressure, and the "capricious" behaviour of the heart under chloroform. The last Commission, that which did its work so well at Hyderabad, and with which all civilised nations must always couple, with gratitude, the name of that high-minded and philanthropic prince, the Nizam of Hyderabad, has taken a strong line of its own, and has declared that it never met with heart-failure under chloroform, either primary or otherwise, and it commits itself further to the statement that it does not believe heart-failure ever occurs. The arguments brought forward are too weighty to be passed over without careful consideration, but are too long for reproduction here.

The weak points in them appear to me to be that their experiments, and the deductions based upon them, fail wholly to explain the cases of Europeans with which we are constantly

meeting, in whom heart-failure does occur. It is asking too much to expect us to refuse the evidence of our senses and our daily experience, because they run counter to the findings of a Commission whose work we all confess was well done, even if its conclusions were fallacious.

The results arrived at by Professor Wood, of Philadelphia, and by Professor MacWilliam, of Aberdeen, and of Professor Gaskell, of Cambridge, are so much out of accord with those referred to above, that we are compelled to admit that the physiological method has not settled the question of how does chloroform kill? It would, perhaps, be premature to say that method never will succeed; but we may, I think, confidently affirm that it must, to be of any real service to us, wait upon the steps of clinical research rather than attempt to settle questions of anæsthesia out of hand, and without reference to our hospital and bedside experience. Further, we have the incontestible fact that deaths under anæsthetics are, if anything, upon the increase, and this in spite of better teaching, the wider ventilation in the journals and in manuals of the most approved means of avoiding fatalities, and a recent propaganda, as far as chloroform is concerned, which seemed to promise immunity from death and danger if certain rules, familiar to most of us, were only kept in their entirety. In spite, however, of all this, deaths go on occurring, and even in the hands of specialists and men imbued to their finger-tips with faith in the innocuousness of chloroform. All this, then, must point to one of two conclusions—that whenever a death occurs it is either the direct result of the anæsthetist's want of skill, or of his lack of conscientiousness—as by allowing his attention to deviate from watching the all-important rhythm of respiration—or that there is something yet in the whole matter of which at present our philosophy has not dreamed. Surely we may reject the former hypothesis, for although Syme in

1855 said that „the differences existing between the methods in vogue in Edinburgh and in London fully accounted for the fact that deaths were rare in the one place, and comparatively common in the other,” yet I think it will be admitted that now-a-days we know every method has supplied its holocaust of victims, and the most skilled and conscientious have lost their patients. If, then, we admit—as I think we must—that there are many points about the practice of the induction of anæsthesia which bristle with difficulties, there remains the question how best to grapple with them, and, if possible, reduce by orderly arrangement and precise investigation their number and formidable aspect. To effect this we may appeal to pathology, the results of necropsies, the clinical study of the effects of anæsthetics in human beings, and the chemistry of the reagents themselves—alike as regards their manufacture and the changes which occur after keeping, exposure to light, &c., &c.

Valuable as no doubt necropsies may be in the future, they have not in the past been of much service to us in elucidating the facts of the question before us. The changes made in the organism by chloroform or ether cannot be dissected by the scalpel, tested by the test-tube, or reckoned in the chemical balance. It may be a satisfaction to the person who gave the chloroform to be able to face the coroner's jury with the announcement that the patient's heart was undoubtedly fatty; but the scientific anæsthetist cannot but remember how many must have been the fatty hearts of patients who passed safely enough the rubicon of chloroform. In the *post-mortem* rooms of any large hospital how many normal hearts do we find? On the other hand, how many are the cases of chloroform-death recorded in the young and vigorous, with hearts healthy, and lungs free from disease? Again, necropsies, to be of any use, should be performed within a

few hours of death under chloroform or ether. When possible, other means having failed to restore consciousness in a person poisoned by chloroform, bleeding to many ounces should be used, the blood being replaced by normal saline or milk, and the blood so withdrawn placed at once in a vacuum over mercury, to be reserved for subsequent gaseous analysis.

I do not wish to depreciate the value of *post-mortem* research, but I attach far less importance to it in the present connection than to the study of our cases during the actual process of the anæsthesia. It may, however, be interesting to inquire what facts have been elicited by the study of the cadaver.

Lallemand, Perrin, and Duroy found in the lower animals the lungs were rose-coloured, but free from congestion, unless a delay had been allowed before the necropsy was made, when hypostatic congestion became well marked.

The right cavities of the heart and veins were filled with dark fluid blood; the left were empty or contained 'clot (Sansom).

Snow, in his careful record of cases of death under chloroform, reports 35 necropsies; 16 showed disease of the heart—*e.g.*, thinning of the ventricular walls, valvular lesions, degenerations.

Turnbull records 44, and of these 20, or nearly 50 per cent., gave evidence of marked degeneration of the myocardium, pericardial, and valvular mischief.

A large number of sporadic cases reported from time to time, but not as yet collected, again afford evidence of the association of heart disease with fatalities under chloroform. It is necessary, however, to accept with caution many of such reports, because in too many cases a preconceived idea existed that such cardiac lesions were sure to be found in any given case.

As regards the other organs of the body, I confess we

know little. The state of the lungs and the kidneys found *post-mortem* shows no causal relation. As one would expect a certain number, which is by no means large, of cases of very grave disease, such as empyema, phthisis, &c., have died under the anæsthetic. This forces upon my mind the belief that we must seek elsewhere for the *fons et origo mali* of death under chloroform, at least in many cases—and that is in the central nervous system. Necropsies do not give us aid here—first, because the skill of the neurologist has seldom if ever been called into requisition in making them; and, secondly, because it is very doubtful, indeed, if the *nerve* cells and commissures have any material impress left upon them by the act of chloroforming. In the physiological laboratory we know that the brain and cord undergo great and palpable changes during anæsthesia, and I have verified as far as possible, in the cases of brain and spinal surgery which I have had with my friend and colleague, Mr. Victor Horsley, the facts which I had learned whilst working with the lower animals. Still these changes, being mainly vaso-motorial, were lost after death.

I may, perhaps, quote a table which Dr. Sansom drew up some years ago which gives an analysis of 51 *post-mortems*, and which draws attention to some salient points in making these examinations:—

Blood:

CASES

Dark and fluid	-	-	-	24
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Lungs:

Congested	-	-	-	18
„ in dependent parts	-	-	-	4
Loaded with blood	-	-	-	5
Normal	-	-	-	10

Heart :

CASES.

Accumulation of fat (right ventricles)	27
Distended - - - -	3
Little or no blood in left ventricles.	
Blood fluid in all but 2 cases.	
In one "air" (<i>sic</i>) was mingled with the blood.	
All cavities empty (in one case "spasm") - - -	9
All cavities containing blood -	13
1 case firm coagulum—galvanism has been employed.	
Auricles empty, ventricles not -	2

Brain :

Congested - - - -	9
Normal - - - -	14
Pale - - - -	7
Air in vessels - - -	1

Let us now pass to the phenomena of anæsthetisation as we meet with them in our daily practice. They are best grouped under the following headings :—

- I. Facts connected with the Patient.
- II. Facts connected with the Operation.
- III. Facts connected with the Anæsthetic.

In the system of recording, which the Committee of the British Medical Association now engaged upon the clinical investigation into the action of anæsthetics has put forth, and books embodying which are now in your hands, an attempt has been made to elicit by questions all the important points covered by these three groups. But before considering the system in detail, it will be well to call your attention to the range of the enquiry upon which you are asked to embark.

All cases in which an anæsthetic is given may be comprised under three headings:—

(a.) Those in which anæsthesia is attained without let or hindrance.

(b.) Those in which symptoms arise which are foreign to the usual course of anæsthesia—*e.g.*, failure of respiration, failure of heart, vomiting, undue struggling, &c.

(c.) Those in which death arises either during the induction of anæsthesia, or within so short a period after its cessation as to be associated casually with it.

Group (a) is important, in that we must have a sufficiently large number of cases of normal anæsthesia to serve as a basis for comparison, and in order that we may learn with greater exactness than is at present possible what are the variations of normal anæsthesia determined by conditions of age, sex, temperament, habits of life, racial traits, hereditary peculiarity, &c.

In dealing with Group (b) we come upon what I am sure will prove the most valuable source of future information, and it is above all things necessary that our records should be exact without being diffuse, and full without being unnecessarily prolix.

If, as is admitted upon all hands, persons die from paralysis of respiration, we should meet with every stage of respiratory paresis; the symptoms carefully observed should guide us into deciding whether the insufficiency of respiration arises from peripheral or visceral, central or nervous causes. In the one case waterlogging of the lungs from bronchorrhœa, in the other paralysis of the muscles of respiration may have been the cause of trouble. Or to take another example—the watchful anæsthetist might find that respiratory movements were being normally executed, but no air entering the lungs, and here symptoms might decide whether the impediment to inspiration arose from a centric cause—namely, spasm—or

was the result of mere mechanical closing of the rima by the falling back of the tongue.

These are but suggestions how a study of respiration during anæsthesia would and does help one; it would be easy, did time permit, to formulate systematically the symptoms which arise and their significance as regards the various anæsthetics. I am accustomed to tell students that were an anæsthetist deprived of hearing, or touch, or smell, yet the remaining sense should be enough to guide him, and should tell him by its use directed upon the patient what anæsthetic was being employed, and what stage of narcosis the patient had reached.

But a close scrutiny of the respiration would soon lead the observer to notice a very close correlation between its force and rhythm and the action of the heart. To me it is a difficult matter to comprehend the strong stand which many most careful and distinguished observers have taken in asserting that chloroform has no primary effect upon the heart. I have seen in my physiological work heart-muscle as muscle absolutely killed by dosing with chloroform, and I have seen the heart of mammals, while in physiological connection with the central nervous system, become enfeebled to final cessation of its action, even when ample artificial respiration was uniformly maintained by air being regularly pumped in and out of the lungs by special mechanism, permitting of no possible chance of asphyxial processes being at work. Again, we know that under the conditions above recited, the whole heart undergoes dilatation. Such being the case it becomes, not only a matter of interest, but one of the most profound importance, that we should make the most exhaustive clinical investigation of the circulatory system of persons whilst under anæsthetics. Thus surely we should remember that the circulation comprises a double heart, an elastic arterial system, and veins and capillaries; we should mentally estimate by the symptoms the state of affairs in the visceral

circulation, the pulmonary system, and last but not least, the vascular system of the brain. This may seem very formidable, and doubtless would be so to the unskilled tyro in precisely the same way that a raw student would find himself perplexed and harried, were he to be directed to work out and record the case of a patient in the manner familiar enough to the careful note-taker in a hospital where clinical work was thoroughly recorded. Still for practical purposes all this can be done by watching *besides respiration*, the force, regularity, and rhythm of the heart and pulse, the colour of the skin, the dilatation of the vessels in the matrices of the nails, the ears, and mucous membranes. The jugular veins give one readily-accessible veins to study, and the colour, moisture, and warmth of the skin again teach us much.

My own observations, made for some years, and without, I think, any bias or prejudice, induce me to believe I have met with all stages of circulatory enfeeblement short of fatal syncope, and that in persons who, to all evidence, were quite free from respiratory difficulties. In these persons I believe we find pallor, cold, clammy skin, sometimes absolute blanching, the radial pulse growing smaller and smaller, and finally ceasing; the cardiac action becoming feeble and flickering; and the respiration only growing secondarily weakened, and then only showing it by regular but progressive shallowing of the respirations. With these I should contrast those who, suffering from asphyxial complications, evidence venous engorgement, marked by cyanosis, irregular, jerky breaths, with an insufficient air entry and exit, and a heart which beats on until, the right side becoming distended, its action grows hampered, and unless a return to the normal respiration takes place, cardiac action ceases. These two extremes, doubtless, meet in the countless intermediate cases which exist, and it is that we may all know the exact delimitations

of such phenomena, that we urge a systematic inquiry into the matter.

These are all practical questions, for upon the recognition of the true nature of the case must depend the method of treatment. What, for example, could be more futile than to administer nitrite of amyl in a case in which respiration has ceased, and the heart is beating well and regularly? Or again, could any proceeding be more likely to remove what chance of recovery a partially asphyxiated patient had than to invert him, and pour more venous blood into a right heart, which was already unable to force its contents through a hampered pulmonary circulation? Would venesection be likely to help a heart which, belonging to an intensely anæmic patient, was, from its feeble resiliency, unable to stand up against the dilating power of the chloroform? But, surely, in a true chloroform toxæmia the abstraction of blood offers a reasonable chance of resuscitating a patient whose death is being compassed by the circulation through his system of a chloroform-laden blood.

We know from Snow's most able researches that 36 minims of chloroform distributed through the circulation of an individual of average weight and health would kill the patient, while 24 would only produce profound narcosis. And while speaking of the possible supervention of syncope, or heart failure, under chloroform, I cannot refrain from noticing a strange oversight which certain persons have committed, who in stating most positively that cardiac syncope never occurs, assert that they are teaching what Syme and Simpson taught, and upholding the truth of a dogma those masters are supposed to have clung to, and handed down to their students. This error a few quotations will rectify. In a clinical lecture published by Syme in 1855 (which, by the way, is erroneously quoted as 1885 in the Hyderabad Commission's Report), he says, "All that I have done has been to

follow the example of Dr. Simpson." While Sir James Simpson describes in the *Lancet* a case in which he himself gave chloroform when the patient died, and he himself attributes the death to syncope from chloroform; and further, in the remarks he made, says: "According to all the experimental and clinical observations which have been made, chloroform appears capable of destroying life in two ways—namely (1), by asphyxia; and (2) by syncope. Death by asphyxia can generally, if not always, be averted by at once arresting the inhalation of the drug whenever the breathing becomes noisy or stertorous—states which, as already mentioned, never occurred with the preceding patient. Death by syncope, or by sudden stoppage of the action of the heart, is undoubtedly far less under control, and has apparently formed the principal cause of the fatal issues in almost all the cases in which the patients have perished when under the use of chloroform."

It would serve no useful purpose for me to go through the problems of life connected with the other visceral systems as regards anæsthetics, but I may state that at present we have no scientific certainty about the behaviour of the kidneys in ether or chloroform patients. The statements that nephritic catarrh follows the exhibition of ether, wants, I think, further proof, and although we have no special column for it in our record-book, yet I am sure it would be a most valuable addition to our knowledge if we got reports of the quantity of urine passed, say, for 48 hours before and 48 hours after each operation, and if an analysis were made both before and after each operation. Again, in my experience glycosuria plays an important part in making our prognosis of the danger of the anæsthetic.

One of the most important issues of the problem before us, is involved in the expression, "after effects." These may be transient and slight, or severe and most serious, since it is

not impossible their severity or persistence may jeopardise the success of the operation—*e.g.*, vomiting after a gastro-jejunostomy. Clinical evidence will, I feel convinced, teach us a great deal concerning the so-called ether bronchitis, the causation of uncontrollable retching and vomiting, and other as important matters. My personal experience convinces me that “after effects” vary directly with the quantity of the anæsthetic given, and are in no slight measure influenced by the way in which it is given.

When we consider Group (*c*) we are met by the statement that so frightful is the loss of a patient under an anæsthetic, that the administrator is the last person from whom any reliable information can be obtained. But I think such a contention need not be seriously considered, for however keenly may the anæsthetist feel his position, he should, if he is a reliable man in his work, remain calm, observant, and ready of resource, and when the danger is passed, or has unhappily culminated in a fatality, his memory should readily enable him to make a clear statement of all that took place during a few minutes of peril.

And we need much carefully-gathered information about deaths which may occur—(i.) in the early stage of narcosis; (ii.) during the deeper stages; (iii.) towards the close or after the completion of the operation. It has been suggested that of the deaths grouped under (i.) that some are due to fright; as, for example, some quoted by Sir James Simpson:—

CASE I.—John Hunter mentions an operation for hydrocele, in which the man was thrown into such convulsions that he despaired of his recovery, and also quotes a man dying from castration. Mr. Travers, in Vol. I. of “Constitutional Irritation,” cites several cases of sudden death in surgical patients.

CASE II.—Robust middle-aged man, moderate-sized aneurysm in femoral artery.

CASE III.—A man who was subject of strangulated hernia.

CASE IV.—A man who had been bitten in finger by cat, died three minutes after excision of bitten part.

CASE V.—Brewer's servant, of robust frame; operation for thecal abscess, caused by splinter of wood penetrating beneath nail of thumb.

CASE VI.—Male; operation for strangulated inguinal hernia, by Dr. John Argyle.

CASE VII.—Strangulated hernia, by Prof. Millar.

CASE VIII.—Child; operation for abscess in neck, by Dr. Pattison.

CASE IX.—In a case belonging to Dr. Gilchrist, of Leith, a surgeon tried to treat a sacculated aneurism at root of neck by a small quantity of hæmostatic injection.

CASE X.—Fracture of radius; adjustment of displaced ends of bone and application of splints and bandages, by Dr. Richard Mackenzie.

Possibly fright accounts for some deaths, not many. The same fear occurs in persons about to be etherised, and yet death in the initial stage of ether narcosis is quite a clinical curiosity; and probably only one, and that a doubtful one, has been recorded. Children, again, seldom die in the first stage of chloroform. Deaths in the later stages of chloroform narcosis usually arise from hampered respiration, or at least this forms a factor in bringing about the result. Diseased conditions of the lungs, especially emphysema, here play a part. The inability to expire freely prevents due exhalation of chloroform, while inhalation goes on.

Clinical evidence will be of the utmost use in elucidating the phenomena of deaths in the second and third classes—*i.e.*, those occurring in the course of the operation, and those taking place after its completion. The problem is only a simple one before the surgeon takes up his knife, for then the anæsthetic or fear is the only cause of death; afterwards.

shock, emergencies such as hæmorrhage, blood entering the lungs, &c., complicate the issues, each probably contributing a factor in bringing about the fatal result.

Most important are the facts which shall serve to elucidate those deaths which take place after the operation is over and the patient is partly or wholly recovered from the influence of the anæsthetic. It is unscientific to put down such casualties to the anæsthetic, speaking vaguely of "ether bronchitis," "heart failure after chloroform," until we have eliminated concomitant circumstances, such as the chilling effects of prolonged exposure, especially of mucous surfaces, the influence of the spray, loss of blood, &c. The only way to work out these points is by clinical research.

Further, among circumstances affecting the patient, we have yet to determine whether the hour of the day, the state of the weather, and electric potential, and so on, do not materially affect the risks of the patient. We know that the vitality of the individual is lower at certain hours—*e.g.*, 3 a.m., and has reached a maximum at 9 a.m.; while late in the afternoon the system is again jaded.

Of sex and age, except as regards proclivity to certain forms of ailment, we know very little as regards their influence on anæsthetics, and the same applies to race and climate.

As regards the nature of the operation much may be said. We know that posture and the surgical proceeding as regards the degree of shock it produces play no unimportant part in chloroform prognosis. Shock, again, in partial anæsthesia—*e.g.*, pulling about of intestines, reducing dislocations, has apparently caused many deaths. It is curious that experiments made upon the lower animals have not been in accord with our experience.

The last points to be briefly considered are those about the anæsthetic:—(i.) purity of drug, its quality, the quantity

employed, and the source and method of its make; (ii.) mode of employment—*i.e.*, the method of administration; (iii.) personal knowledge of anæsthetist, and his responsibility.

There can be little doubt, I think, that a certain number of deaths under chloroform must be put down to inexperience or carelessness on the part of the administrator, and probably impure chloroform, may account for some others, so that the headings given above cannot be held to be superfluous in the conduct of any clinical investigation into the action of anæsthetics.

In conclusion, I beg to thank you for the courteous hearing you have accorded me.

The PRESIDENT of the Royal College of Surgeons said he was asked by Dr. Childs, of Weymouth, as Secretary of the Anæsthetics Committee of the British Medical Association, to form a sub-committee in Ireland in connection with the Anæsthetics Committee, and he hoped to do so as soon as possible. The President hoped the President of the Royal College of Physicians, and Fellows and Members of his College, would co-operate. Also the Dublin Branch of the British Medical Association, and the Royal Academy of Medicine, the Infirmaries, and Workhouses, and Dispensaries. In fact, the anæsthetic question was one of vital importance to every member of the profession, no matter where located, and also to each individual who takes an anæsthetic. He drew attention to the greater risk attendant on the administration of anæsthetics in cases of minor operations—and reductions of dislocations than in major operations. He was thankful to say he never was present at a death from an anæsthetic, but he saw several "touch-and-go cases." He confessed he was always more afraid of the anæsthetic than the operation.

MR. ORMSBY stated he hoped something definite might come from this committee, but he greatly feared if the matter was not in the hands of an experienced committee, with two local hon. secretaries, who had specially investigated the subject of anæsthetics, and who would really take trouble about the matter, very little good would be done. As an operating surgeon he always feared the anæsthetic far more than the operation, and he considered that if the investigation was carried out in a thorough manner, it would prove a valuable record. As the paper did not

allow any discussion on the relative safety of one anæsthetic over another, he would conclude his remarks by congratulating Dr. Dudley Buxton on his very able and practical paper.

Mr. THOMSON was of opinion that much good would result from the work of the committee which it was proposed to appoint. He agreed in the importance of instruction in the methods of administering anæsthetics, all of which had dangers; and he was afraid that everywhere the profession had failed in its duty in this respect. The paper did not permit any discussion on the merits of various anæsthetics, but as to the causes which were supposed to lead to death, he thought there was to be added the influence of fright which was produced by the publication in the ordinary newspapers of reports of deaths from anæsthetics.

DR. HARLEY.—I fear there has been a panic about anæsthetics displayed here to-night quite uncalled for, and I was sorry to hear the President of the College of Surgeons state that he approached an operation with more fear of the anæsthetic than of the operation. I have given anæsthetics frequently for the late Mr. Butcher, Mr. Wheeler, and other surgeons, also for obstetric surgeons, and have had patients anæsthetised for from an hour and a half to two hours for Emmet's operation, Hagar's dilatation, and such others, besides many capital operations, and I am happy to say the operating surgeon never had any anxiety about the patient. I have never seen a death from anæsthetics, nor have I ever been frightened about a patient. I have never even had a case of what Mr. Croly calls "touch-and-go." I have been twenty years practising, and I have never heard of more than two or three deaths in all that time from an anæsthetic. In private practice I certainly always devote my whole attention to my own duty, and never think of the operation.

Mr. FOY.—Dr. Dudley Buxton's paper gives very little scope for the discussion of the method of administration of anæsthetics. As regards the relative values of chloroform and ether as anæsthetics, we are precluded from discussing it. The author has stated that deaths from chloroform were still increasing, but he does not mention that the percentage of deaths is less. Outside London, with the exception of New England States of America, chloroform is the favourite anæsthetic. The risk referred to as due to emphysema might be avoided by inhalation of oxygen. The advocacy of certification cannot be approved of; there are too many certificates. What is required is sound clinical and theoretical teaching

on the subject, and a searching examination of candidates on their risks. Climate and race cannot have any effect other than the slightest on chloroform. It is used by Semites in India and Aryans in Scotland with equal success.

SIR WILLIAM STOKES fully endorsed the opinions expressed by the preceding speakers as regards the importance of the communication of Dr. Dudley Buxton, and thought that it was of the utmost importance that the administration of anæsthetics should not be entrusted to inexperienced persons, and also to the necessity of employing pure anæsthetics. He, however, expressed some disappointment that a sufficiently definite line of investigation was not indicated by Dr. Buxton in the proposed inquiry, and feared that if the results of the labours of the Committee about to be formed were to be nothing but a record of cases to which anæsthetics had been administered, the statistics would not be attended with better results than those of the collective investigation of disease, which were eminently unsatisfactory.

DR. MYLES was of opinion that the method proposed was simply a collective investigation committee; that the results, as in similar cases before, would be merely a certain amount of temporary notoriety for the leaders of the movement. Dr. Myles further pointed out that very little reliance could be placed on the observations of untrained men, and that the experimental method was the only one from which reliable deductions could be drawn.

MR. F. A. NIXON.—I must take exception to the statements which have been made to-night:—1st. That the Profession in Dublin are in any way careless in the administration of anæsthetics. This is a grave reflection upon them, and I may say for myself, that the duty of giving an anæsthetic is always to me a subject of great anxiety, and one which occupies my *entire* attention. 2nd. That those who administer anæsthetics are ignorant, inexperienced, and do so without any instruction on the subject. This is, indeed, a grave reflection on the clinical teaching of the Dublin School. I wish, again, to say for myself, that it has always been my practice to carefully instruct students in the administration of the anæsthetic before and during its administration; to draw their attention especially to the condition of the respiration, the pupil, the pulse, the appearance of the face, &c., and this I consider to be one of the most important duties of every clinical teacher, which I regret to hear to-night has been so much neglected.

DR. DUDLEY BUXTON, in reply, said: The primary object of his

paper was to urge upon his hearers the importance of clinical observation of cases in which anæsthetics were given. The question which anæsthetic was safest would follow as a sequel when the inquiry was completed. That collective investigations had failed was no doubt in part true, but the present inquiry was not being carried out on the lines of the collective investigations, and could hardly fail to bring to light a vast mass of valuable information. Sir William Stokes had expressed disappointment that no definite line of investigation had been set forth, but Dr. Dudley Buxton pointed out that Sir William Stokes was in error, for a most definite line had been carefully worked out by the Anæsthetics Committee, of which the speaker was one of the executive, and was set forth in the books placed in the hands of Mr. Croly for distribution. This being so, the speaker had refrained from occupying the time of the meeting by reiterating the particulars of the scheme. As to whether anæsthetics were properly taught in Dublin he (the speaker) was unable to say. He was glad to hear from Dr. Nixon that the teaching was so excellent, for he felt Dublin must be an exceptionally favoured city. The gentleman who feared panic must be reassured by Dr. Nixon's remarks, and the other speaker who had never met with any case of chloroform narcosis which gave him anxiety was to be congratulated upon an almost unique experience if, as he seemed to imply, he had had some little practice in the use of that drug.

NOTES OF A CASE OF RUPTURED UTERUS.

By P. J. BARRY, L.M., L.R.C.P., L.R.C.S. ;

Senior Assistant Physician to the Coombe Lying-in Hospital.

[Read in the Section of Obstetrics, February 12, 1892.]

THE importance of the subject of rupture of the uterus is so generally admitted that I venture to bring forward the notes of a case occurring in the extern practice of the Coombe Lying-in Hospital. I was called to the case by one of the students of the hospital, and on my arrival obtained the following history:—Patient about thirty-five years of age, primipara; thirty-eight hours in labour, having strong labour pains from the onset to within two hours of my arrival. Liquor amnii had drained away early in labour. The patient presented a pinched, anxious appearance, with a cold, clammy, sweat and a quick, weak pulse. On inspection abdomen presented nothing abnormal; carefully auscultating over uterus no foetal heart could be detected, although heard some hours previously. By palpation a vertex could be made out at the brim of the pelvis. On making a digital examination, vagina hot and dry; os uteri about the size of a florin, thick and rigid. On passing my finger through the os uteri, I found a tear extending in an oblique direction for about four inches. There was no prolapse of intestine; promontory of sacrum easily felt, with vertex presenting at the brim of the pelvis. Conjugate diameter measured roughly about three inches; some sanguineous discharge from vagina. Having passed a catheter, and drawn off about a pint of dark-coloured urine, I then got an assistant to steady the uterus, keeping pressure in the axis of the brim of the pelvis; with a probe-pointed bistoury incised that portion between os uteri and the tear. Applied forceps, and after making the most powerful traction that I could, failed to engage the head in

the pelvis. By perforation I effected an easy delivery of the head, but experienced considerable difficulty with the shoulders. The placenta, which was morbidly adherent, was attached to the fundus, and slightly posteriorly. There was fair uterine contraction, and very little post-partum hæmorrhage. The patient had complete retention of urine, which condition persisted until the sixth day after delivery, when she regained perfect control over her bladder, the catheter having been passed twice daily for that time. Ten hours after delivery the student having charge of the case, making inquiries if the patient had passed water, was informed by her friends that the water was coming with the "discharge" during the night. But when I saw the patient a short time afterwards, I found the bladder distended, and drew off 20 ozs. of healthy-coloured urine (by measurement). The watery discharge coming with the lochia continued for two days. I look upon this watery discharge as peritoneal exudation, coming through the rent in the lower zone of the uterus.

Treatment.—Ergot and liq. strychninæ for first three days, then sulphate of quinine in large doses to reduce temperature, with mist. diaphoret. Potassii brom. gr. 40 at night to procure sleep. Vagina douched out twice daily.

This patient visited me at the dispensary, Nov. 24, 1891, three months after delivery. On examination the uterus was found normal in size and position; patient menstruating naturally without pain. No pelvic pain or backache; the os uteri quite small, scarcely admitting the point of the sound. And neither by digital examination, nor by the aid of the speculum, could I either feel or see a cicatrix corresponding to the tear.

Rupture of the uterus is one of the most appalling complications met with in midwifery practice, and one also difficult to foresee or control. Up to the beginning of the present century cases of ruptured uterus were practically abandoned

as being beyond human aid. To Dr. Douglas belongs the credit of being one of the first in Great Britain to resort to artificial delivery, he having reported a successful case in 1784—active interference practically dates from that time. Many difficulties are encountered in the diagnosis of this complication, and quite a number are overlooked till after delivery. The majority are, however, easily recognised. The patient herself is conscious that something has gone wrong. She suffers from acute localised abdominal pain; her appearance is anxious; cessation of labour pains; recession of the presenting part; hæmorrhage, shock, a quick, weak pulse; a cold, clammy, sweaty dyspnœa. The death of the fœtus as a rule quickly ensues. The immediate result of the case depends on the amount of hæmorrhage; it is sometimes so great that the patient quickly dies before assistance can be rendered her. In a number of cases where the hæmorrhage is not excessive, or the shock profound, the prognosis, though grave, is much more favourable.

Dr. Trask, in his monograph published in the *American Journal of Medical Science*, 1848, records some very interesting cases:—

CASE XLVIII.—A stout young woman; rupture in severe labour, which lasted thirty hours; nearly four feet of intestine protruded through the rent, and sloughed off on the sixth day; fæces voided per vaginam for two years, when they took the natural channel. Eighteen months after this she conceived and had a living child.—Dr. McKeever, *Lancet*, 1828.

CASE LXII.—Patient, aged forty-four, mother of five children; labour having commenced; whilst standing up became faint, and vomited; had a sense of laceration, and feeling as if there were two children in the abdomen; abdomen swelled; vomiting continued; breathing irregular; os undilated; two hours after rupture laparotomy; child extracted alive. Patient recovered in forty days.—Dr. Frank, 1825.

CASE LXX.—A woman of Toulouse had rupture during the

pains of a very severe labour. Fœtus passed into the abdomen, and remained for twenty-five years. After her death the fœtus found invested in false membranes, and the rent by which it had escaped distinctly visible.—Bayle.

CASE LXXIII.—Rupture and escape of the fœtus into the abdomen; became pregnant a second time; extra-uterine at the end of seven years, during which time the fœtus was in the abdomen. At the end of twenty-one years she began to void the bones of both per vaginam; the discharge continued during eighteen years.—Underwood.

CASES LXXXII., LXXXIII.—The wife of a vine-dresser; rupture; laparotomy at the end of eighteen hours; the child dead; a gangrenous abscess formed in the hypogastric region, but she was at work in the fields in six weeks. At the end of nine years pregnant again, with rupture of the uterus; fœtus escaping into the abdomen; laparotomy at the end of two hours; fœtus gave signs of life for a half an hour after the operation. The woman subsequently had a child naturally.—M. Lambron.

Fourier records a case of rupture of the uterus, with escape of the fœtus and placenta into the abdominal cavity, on which he performed laparotomy on the fifth day. The patient was twenty-four years of age, the mother of two children. Six weeks before her expected labour she fell going down stairs. This, however, gave rise at the time, and for a few days afterwards, to only slight abdominal pains. Labour came on at term, and soon after the rupture of the membranes she felt a sudden and severe pain, and there was a cessation of labour pains. On careful examination four days afterwards, the abdomen presented two tumours. The one on the right was made up of the fœtus, whilst that on the left was the size of the uterus soon after delivery. An incision was made on the right side; fœtus with membranes and placenta extracted; patient recovered. There was absence of hæmorrhage—only a slight gangrenous discharge shows after rupture. The presence of the fœtus and placenta in the cavity did not give rise to peritonitis.

In conclusion, I would urge the following points as to treatment :—

1. (*a*) If the fœtus has escaped into the abdomen; (*b*), if the hæmorrhage is severe, and cannot be controlled; (*c*), or if you have prolapse of intestine, abdominal section is indicated.

2. Deliver your patient as speedily as possible, and by that method which affords her the least risk :—(*a*) If the fœtus is still in utero, os dilated, vertex presenting, deliver by forceps, or if the child is dead, by craniotomy; (*b*) breech presentations, if still in utero, deliver manually; (*c*) shoulder presentations, by embryotomy or abdominal section.

3. It is dangerous sometimes to wait for reaction, as shock and collapse may be due to hæmorrhage, which if not controlled will quickly dispose of your patient. Give stimulants by the mouth or rectum, ether hypodermically.

The fœtus dies soon after rupture, and, as a rule, does not enter into our consideration. But should we get signs of life, I would advocate abdominal section in preference to craniotomy. In the present advanced stage of abdominal surgery, and the good results achieved by laparotomists, I do not see why the mortality from this grave accident should not be considerably reduced.

I have brought forward this case first of all that it may be recorded; secondly, that it may elicit some remarks from the senior members of this Section whose field of observation is extensive, and whose experience must be relatively great, and not that I had myself anything new or original to impart.

DR. ATTHILL said the case as described by Dr. Barry was a remarkable one, and as far as he (Dr. Atthill) was aware, no similar one had been recorded. Here was a woman 38 hours in labour, and in whom the waters had long before drained away, with so small a conjugate diameter that the head never came in contact with the os, yet a rent occurred in a position so situated that the finger passed into the os could detect it; it evidently was a rupture of the cervix. How it could have occurred under the conditions detailed Dr. Atthill was unable to explain.

NOTES AND REMARKS ON A CASE OF HYSTEROMYOMECTOMY.

By F. W. KIDD, M.D. ;

Late Assistant Master, Coombe Hospital.

[Read in the Section of Obstetrics, March 18, 1892.]

IN these advanced days of abdominal and pelvic surgery, when such a case as that which I now submit to you is no longer the rarity it would have been in Dublin some four or five years ago, I feel that I should almost apologise for bringing it in the form of a paper before the members of this Section were it not that the recounting of my errors and difficulties might be of service to some of the junior members if they had to deal with such a case, and that I hope the details may evoke valuable criticism and advice from the senior members who have had opportunities of dealing with similar cases.

The previous history of the case is as follows:—

Last May I was requested to see the patient, who at the time was suffering from a gastric and bilious attack. I made the following notes:—"Unmarried, age thirty-two, rather under average height, complexion florid, general health fairly good, suffers occasionally from constipation and bilious attacks—often dyspeptic; menses, until four months ago, were regular at monthly intervals, painless, lasting three days; some premenstrual leucorrhœa for two days. Since January menses have come at intervals of five weeks, lasting five to seven days, more profuse and accompanied by pain, both in the lower part of abdomen and at sides. On external palpation a tumour was discovered extending from the pubis to umbilicus—semi-elastic, movable; when patient lies on her back

it seems to encroach more on the right side than on the left; slight tenderness at upper and right side of tumour. Patient denied the knowledge of the existence of this tumour until her attention was drawn to it on this occasion. Stethoscope revealed no murmur. Measurements as follows:—From symphysis to upper border of tumour, 21·5 cms., or 8½ in.; circumference at umbilicus, 71 cms., or 28 in.; from umbilicus to right ant. super. spine of ilium, 15·25 cms., or 6 in.; from umbilicus to left ant. super. spine of ilium, 14 cm., or 5½ in. Very little information could be gained by a vaginal examination, the hymen being intact; the introitus vaginæ was very small; the abdominal walls were very tense, and the patient, moreover, was very nervous. The cervix, which was well formed, was a little lower than usual in vagina, and sound passed 2¾ inches. Uterus could be raised by pressing on cervix, but seemed to raise tumour with it. Tumour gives feeling of elasticity; no fluctuation; clear intestinal percussion note to either side of tumour. Tumour was not rotated, while sound was in uterus on this occasion. She complained of more discomfort and pain on the right side than on the left.

The diagnosis seemed to lie between a multilocular ovarian tumour, with uterine adhesions, and a uterine myoma; to the latter view I inclined at that time.

Subsequently, in Sept., Dr. G. H. Kidd kindly saw her with me. On this occasion, although less nervous than formerly, it was extremely difficult to make a satisfactory examination. The sound passed 2¾ inches, and the handle of the sound, when the tumour was rotated, only seemed to move as much as if the motion was communicated and not direct. Dr. Kidd inclined to the belief that the tumour was ovarian, and on this occasion I agreed with him. Since the occasion of the first examination the menses had occurred at even longer intervals, were more profuse and accompanied by more pain.

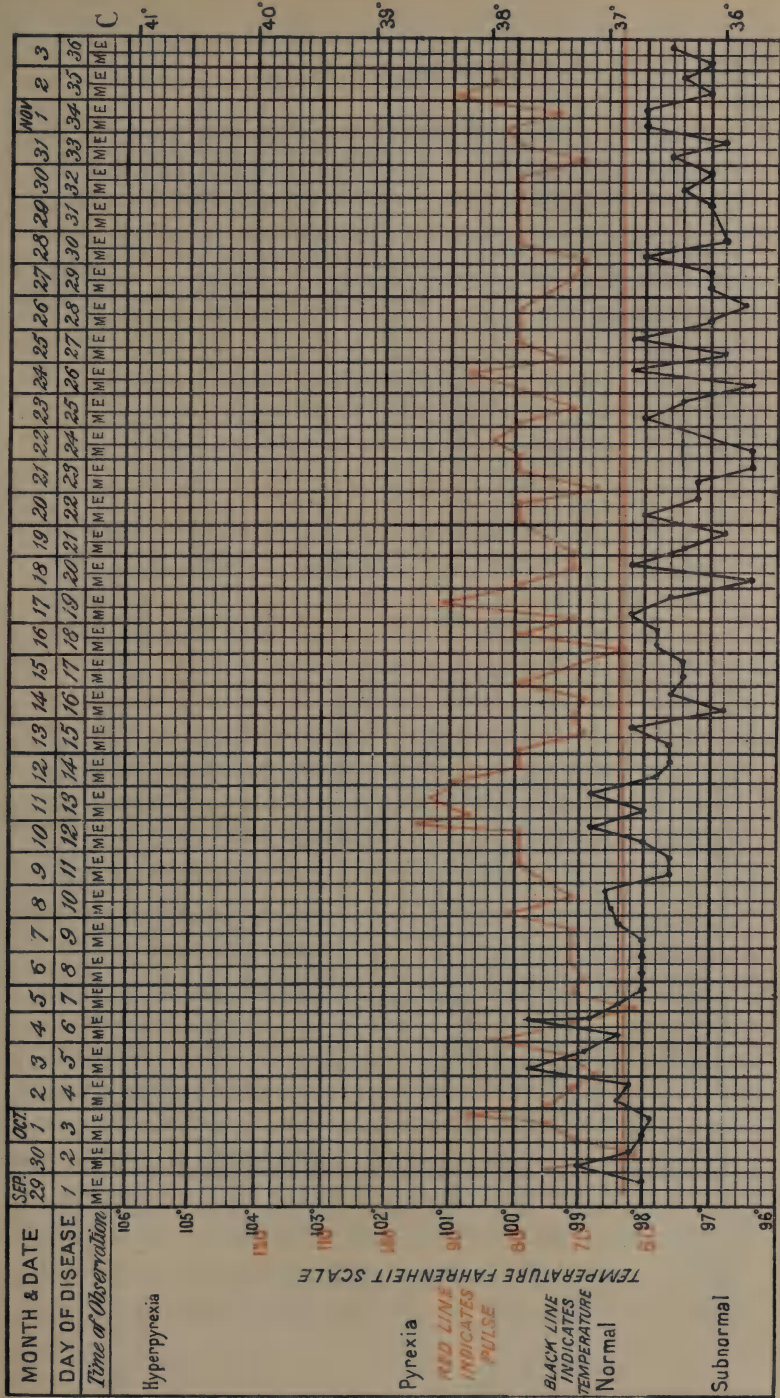
The tumour had visibly and sensibly increased in size, her dresses no longer fitting her; it had given her much discomfort, irritating the bladder and causing frequency of micturition; it also, from its weight, interfered with her walking much. She said she was "never comfortable." The measurements had increased, that from the pubis to the upper border of the tumour from 21·5 cms. to 25·5, and the circumference at the umbilicus from 71 cms. to 76 cms. Tumour continued to trouble her more on the right side. Believing it to be ovarian, she was advised to have it removed, and to this she consented. The operation was performed on the 29th September, Dr. Wm. Smyly kindly assisting, while Dr. Harley gave ether. Dr. G. H. Kidd was unfortunately, at the last moment, prevented from attending. The patient had been prepared in the usual method. The bowels were thoroughly cleared out, the symphysis pubis, always a favourable nidus for bacteria, had been shaved, patient had a hot bath the preceding evening, and a wet pack of carbolic solution placed over the abdomen for the night; heart and urine were examined and found normal. All the preparations were carried out with aseptic precautions. When the incision had been made and the peritoneum opened, there was no ascitic fluid. The tumour did not present the usual smooth pearly appearance of an ovarian cyst, and on enlarging the incision so that the surface of the tumour could be followed down into the pelvis, it was found that the body of the uterus was incorporated with the tumour. Acting on Dr. Smyly's advice I proceeded to remove the tumour and uterus. Having come unprovided with a transfixion pin and serre-nœud, Dr. Smyly sent for them to the Rotunda, knowing that they would be aseptic. In the meantime the incision was carried downwards, as far as possible with regard to the safety of the bladder; it was also continued upwards to the left side of the umbilicus for a distance of two inches, which

had to be still further increased before the tumour could, after repeated efforts, be turned out through the wound; there was, up to this point, little or no hæmorrhage. The right broad ligament was the first to be ligatured and separated; it was tied in three portions. During this procedure a large vein bled profusely, whether injured by the pedicle needle in passing one of the ligatures, or by the tension caused by drawing forward the tumour, or by both causes together I know not, but it was quickly secured. The left broad ligament was then treated in a similar manner. The elastic ligature was applied so as to clear the tumour, and the delta metal transfixion pin was introduced. The tumour was then cut off, leaving a stump which was formed by the lower portion of the uterus, the cavity of which had been cut through. A great deal of blood drained from the tumour as it was being removed, but the elastic ligature completely controlled any hæmorrhage from the stump. The cavity of the uterus in the stump was freely cauterised with Paquelin's cautery, the surface of the stump being also touched over, so as to assist in keeping the surface of the stump as dry as possible. The peritoneal cavity was then carefully cleansed of all clots, &c., and the wound closed. The first sutures inserted—namely, those immediately above and below the stump—were made to catch up the peritoneum on the stump with the tissues and peritoneum on each side so as to occlude or seal off the peritoneal cavity above and below from any possible contamination from offensive discharge. There were nine deep sutures and six superficial—silk being used which had been previously boiled in carbolic solution and then kept in absolute alcohol. The length of the incision, when the sutures were all in, was 16·5 cms., or 6½ inches. It had been intended to use Kœberle's serre-nœud, but portion of it had been dropped in transmission from the Rotunda, so the elastic ligature was left. The stump was dressed by slipping

all round it strips of iodoform gauze; it was then powdered with a preparation composed of three parts of tannic acid to one part of salicylic acid, which had the double advantage of being antiseptic and of drying the stump. No drainage-tube was used; the abdominal incision was powdered with boracic acid and covered with iodoform gauze. The abdominal walls were supported with strips of adhesive plaster, a good pad of alembroth wool being placed over all, under a binder. As regards the extra-peritoneal treatment of the stump, I may here mention, quoting from a paper of Dr. Bantock's, on Fibroid Tumours, that Dr. Martin, of Berlin, who had a very extended experience of the intra-peritoneal, has abandoned this operation for that of complete extirpation of the uterus.

Although the operation took considerably more than two hours to perform, there was no collapse, the temperature at 7 p.m. being 98° and the pulse 76. It is unusual not to have collapse after a prolonged operation of this nature, and I attribute its absence to the great care which was taken to keep the intestines warm with hot sponges and to expose them as little as possible. The patient was not given anything except an occasional teaspoonful of cold water until 12 o'clock the next day, when she got 3i Denaeyer's meat peptones; this, with whey, peptonised milk, and Caffyn's Liquor Carnis formed her diet for three days, when she got bread and milk, fish, &c. The catheter was passed every six or eight hours for the first week, after which she passed water herself. I do not believe that catheterisation is productive of as much harm as some authorities think, if proper precautions are used to ensure the instrument being perfectly aseptic; and for this reason I think the glass catheter with females is the safest to use. Constipation was one of the troubles during convalescence, and had to be regulated by such means as calomel, magnes. sulphat., colocynth and hyoscyam.,

Operation, Hysteromyomectomy,



cascara sagrada, and *enemata*. Indeed at one time there was so much trouble in getting the bowels to act that I feared some band of organised lymph might, by constricting the bowel, be the cause; but I am glad to say these symptoms gradually disappeared. She was troubled with flatulence and once or twice with vomiting, but this seemed to depend more on the weak state of her stomach than on anything connected with the operation. The wound was not dressed for a week, the abdominal incision was found completely united. There was some discharge about the stump, which subsequently was dressed every second day until the eighteenth day, when the pin and stump came away, leaving a granulating cavity with a small yellowish-looking slough. This sinus was dressed and syringed every day, and the slough came away with elastic ligature four or five days later; sinus did not close until two of the abdominal ligatures of strong silk were discharged, at an interval of eight days between them. She was supplied with an abdominal belt and pad and left the private hospital on the 17th November. She took a month's holiday in the country, and has continued quite well ever since. The temperature never rose above 100° , as will be seen by referring to the accompanying chart, while for three weeks or more the temperature remained subnormal, sometimes registering as low as 96.8° . This temperature was verified by having it taken in the mouth and by different thermometers. This period was synchronous with the time during which the abdominal sinus remained open and before the last ligature was discharged.

This clinical record of temperature seems very interesting to me, and it certainly seems to depend on the presence of suppuration caused by these ligatures that came away and kept the sinus open, as the temperature rose to normal when they were discharged and the sinus healed. There was no pain or inflammation at the seat of this suppuration, nor

during this period was the patient at all depressed; she was cheerful, and ate and slept well—in fact was up and walking about. While the temperature was so low the patient was given stimulants and had hot water bottles in her bed. I would wish to have the opinion of some of the senior members of this Section as to the causation of reduced temperature in the presence of suppuration. I think it is a point on which too little stress has been laid in the reporting of cases.

On looking at the specimen here shown, the upper portion of the cavity of the uterus can be seen, the cavity in this portion only measuring about $1\frac{1}{2}$ inches in depth, so that there could have been no error in the original measurement of the cavity. It seems somewhat unusual that the cavity of the uterus should be so little enlarged considering the size of the myomata growing in connection with it. The section through the tumour shows that it is mainly composed of two large myomata that have amalgamated, and which cause a dip in the surface which might have been misleading, and simulated the feeling of a multilocular cyst. The tumour seemed much more elastic when recent, and, of course, contained a great deal of blood. When its presence was first discovered, the patient, in answer to inquiries, persisted in stating that she was never previously aware of its existence, and on more than one occasion since, reiterated this statement, although at the time the tumour was fully the size of a foetal head at full term. This point is worthy of attention as it seems strange how a well-educated woman, observant in other matters, could overlook such an obviously important physical change. Yet such cases are well known amongst women whose veracity is unimpeachable, and who could not possibly have any object in practising deception. By our mistakes more than by our successes are we likely to acquire knowledge; and in reviewing this case, certainly the initial

mistake was that of diagnosis. There is no doubt that there are cases so difficult to diagnose that it is not until the abdominal incision has been made—and sometimes not even then—that the diagnosis becomes perfectly clear; but in my case I think that had the patient been put under chloroform solely for the purposes of examination, points would have been revealed that might have rectified the diagnosis, and it is my determination in future always to insist on an examination of the patient under an anæsthetic where the slightest doubt exists before subjecting her to a serious operation. (I must, in justice to Dr. Smyly, say that he did not make any examination of this patient prior to operation.) Granting that the diagnosis of myomata had been made, would I have been justified in recommending operation, and if so, what operation? This patient gained her livelihood as a governess; she was suffering from discomfort, pain, hæmorrhage, and felt unable to attend to her duties, and all these symptoms developed within a very few months. The rate of growth was very rapid. Was not an operation indicated? Would not her condition—presuming these symptoms increased—have been a great deal more unfavourable for operation later on? The operation for the removal of the appendages, with the hope of arresting the growth of the tumour, might have been entertained, but in this case such an operation would have been very difficult, if not impossible, owing to their position at the back of this tumour, making them almost impossible to reach. Moreover, does removal of the appendages arrest the growth invariably? Tait says it succeeds except in the case of a soft oedematous myoma.

Another error was one of omission—namely, the fact of going to an abdominal section unprovided with a *serre-nœud* or a transfixion pin. Such an omission might have resulted in not being able to complete the operation when the

abdominal incision had been made. The invariable rule should be, whenever there is room for any doubt, to come provided to deal with the unexpected, as even the most experienced abdominal surgeons have made errors in diagnosis. I take this opportunity of returning my sincere thanks to Dr. Smyly for his invaluable assistance and advice during a long and trying operation, and also to Dr. Harley for the manner in which, by the successful administration of the anæsthetic; he never caused me one moment's anxiety as regards the condition of the patient.

If by remark or criticism on this paper, information be afforded which is useful or valuable to members of this Section, its object has been fully realised.

DR. ATTHILL said that though errors of diagnosis could hardly be entirely avoided, still their occurrence would be rendered much less frequent were the patient examined while under the influence of an anæsthetic, as should happen in all doubtful cases. In Dr. Kidd's case the patient suffered from profuse menstruation, and that symptom pointed to the probability of the tumour being uterine, menorrhagia being of rare occurrence in ovarian disease; also the frequent desire to micturate is seldom observed in the latter, but is often complained of by patients who are the subjects of uterine myomata.

DR. KIDD replied. In reference to what Dr. Atthill remarked about the probability of the tumour being uterine because of the occurrence of profuse menstruation and bladder symptoms, Dr. Kidd held that, as the menstrual periods were at longer intervals than usual, and as the tumour was diagnosticated as being ovarian with uterine adhesions, the element of doubt still remained, as prolonged intervals between menstruation and bladder irritation might be caused by an ovarian tumour with adhesions as well as with a purely uterine tumour. Dr. Kidd regretted that none of the members had touched on the point of the subnormal temperature for three weeks as being dependent on the suppuration caused by ligatures being expelled.

FURTHER OBSERVATIONS ON THE CAUSES AND TREATMENT OF DYSMENORRHOEA.

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IN none of the many branches into which modern medicine has become subdivided has such rapid progress been effected during the last decade as in that within which the special study of diseases peculiar to women is included. Nevertheless, this development might probably have been still greater were it not for the occasional evanescent prevalence in matters gynæcological of theoretical dogmas which are not borne out by subsequent experience, or by the attempted displacement, on possibly inadequate grounds, of well-established doctrines, such, for instance, as those heretofore generally accepted concerning the ætiology and treatment of difficult or painful menstruation.

Under these circumstances, the rediscussion of even the most familiar subjects connected with the practice of gynæcology may from time to time become an inevitable necessity, and at the present date that appears to me especially the case with regard to the causes and treatment of dysmenorrhœa, one of the forms of which was brought under the notice of this Academy in a communication of mine a few years ago. Since then some more recent doctrines in reference to this complaint have been propounded on high authority, which, if generally accepted, must to a large extent revolutionise the ideas and methods of treatment heretofore generally adopted

as to this complaint. Before that consummation is arrived at it may be well that those statements, and the practice founded thereon, should be carefully considered and tested by a comparison of the clinical experience of gynæcologists who have not as yet, at least, seen cause to recant their earlier teaching, with that of those who have been converted from the possible errors of their former ways in this respect. Therefore, I now submit to the Academy the following recapitulation of some of the points now controverted with reference to those views, by acting on which, as I think, the most satisfactory practical results may be secured in the treatment of dysmenorrhœa.

Pathological Importance of Dysmenorrhœa.—Of all those disorders of menstruation which furnish a large proportion of the gynæcological cases treated in the hospitals with which I am connected, dysmenorrhœa is that most frequently noted, not only at the earlier catamenial periods, but also in later or married life. In the former this trouble commonly comes before us in association with one or other of the manifold reflex or sympathetic nervous or constitutional derangements traceable to the imperfect accomplishment or evolution of the catamenial function in cases of difficult or painful menstruation; whilst in subsequent years it is no less often brought under clinical observation as one of the evidences of some of the local causes of infecundity. In regard, therefore, not only of the intensity of the immediate suffering attending well-marked dysmenorrhœa and the complexity and possible gravity of its consequent reflex nervous and constitutional disturbances, but also in view of its direct bearing on the fertility of the patient, the now disputed ætiology and treatment of this disorder, however inadequately they may be introduced by the present writer, are entitled to the careful consideration of this Academy.

Frequency of Painful Menstruation.—As already observed,

dysmenorrhœa is brought under clinical observation more frequently than any other catamenial derangement. Thus, for instance, in the last 1,000 gynæcological cases of every kind noted in the out-patient department of my hospital, in 189 instances some menstrual disorder was especially complained of—viz., dysmenorrhœa in 110, amenorrhœa in 42, and menorrhagia in 37 cases. These figures, although they cannot be perhaps regarded as representing the exact proportion of each of these complaints (inasmuch as, whilst many women may long suffer from amenorrhœa or menorrhagia before obtaining medical advice, few patients can long withstand the periodic recurrence of the tortures of acute dysmenorrhœa without seeking relief), nevertheless at least show the greater frequency with which, in the instances referred to, such cases were brought under my clinical observation, and help to prove the fact that dysmenorrhœa is here, as elsewhere, the most frequent of the catamenial complaints that come before us.

Classification of Dysmenorrhœal Cases.—In the clinical records of the hospital on which the foregoing statement as to the frequency of dysmenorrhœa is founded, it was deemed advisable to classify in some way the different cases and forms of dysmenorrhœa there noted, and for this purpose their ordinary subdivision under the three headings of “neurotic, congestive, and obstructive dysmenorrhœa” was followed. But however convenient for reference or description any arrangement such as this may be, it must at best be merely arbitrary and often inapplicable, being obviously founded on the greater or lesser predominance in each instance of one or other of the special symptoms of the coexistent neurotic or inflammatory condition, or of those still more directly due to local and mechanical causes. It, therefore, cannot be too explicitly stated or too often reiterated that in every case and form of dysmenorrhœa there is one common factor in

the causation of the complaint to be reckoned with—viz., some obstruction, physical or mechanical, to the free escape of the catamenial outflow, which impediment may be due either to the condition of the parts concerned in the menstrual function, or to the character of the resulting discharge; and on the recognition and removal of that obstacle, whether by constitutional remedies, as may be possible in certain instances, or by local measures, as is necessary in a far greater number of cases, will be found the key to the general pathology and successful treatment of painful menstruation. This general statement is, I think, equally applicable to every variety of dysmenorrhœa, not only in its uterine forms, to which the following observations are chiefly confined, but also to those no less important cases in which the causes of the complaint are traceable to the condition of the ovaries or of the Fallopian tubes, and to the consideration of which I hope, possibly, to be able to return on a future opportunity.

Obstructive Dysmenorrhœa.—On this subject my experience and opinions are wholly at variance with those of Dr. Champneys, who, in his “Harveian Lectures on Puerperal Menstruation,” expresses very forcibly his incredulity with regard to the considerable influence heretofore commonly, and as I believe correctly, ascribed to mechanical obstructions from flexions or stenosis of the cervical canal as causes of spasmodic dysmenorrhœa. “Until the last few years,” he says, “the question of the pathology of spasmodic dysmenorrhœa was considered settled by a very lucid and intelligent explanation known as the ‘mechanical system of uterine pathology.’ The theory of this pathology was that it was owing to obstruction to the escape of the menstrual fluid, due to flexions of the uterus or to stenosis of some part of the canal. Other elements are cited in the shape of the texture of the uterine tissue and disorders of the uterine circulation by flexion. They are thus summarised—(a) Flexion of the uterus causing

obstruction; (b) Stenosis congenital, due to swelling of the mucous membrane, fibroids, polypi, hypertrophy, increased flow of blood; (c) Chronic congestion of the uterine walls, due to flexion." . . . "We have now gone through the whole series of statements in favour of the mechanical theory of dysmenorrhœa, and have found that most of them are at variance with facts as ascertained, while many of them are founded on interpretations which are capable of being reviewed from another side, and are most probably erroneous. It seems to me that had the facts as regards flexions, stenosis, and the uterine circulation been ascertained before propounding a theory, the mechanical theory of dysmenorrhœa could never have been propounded. To ascertain the facts that have now rendered it untenable has been the work of years; but this is not the only instance in medicine of the labour entailed on innocent persons by the promulgations of a theory on insufficient grounds. Some of the most important work has appeared in the *Archiv für Gynäkologie*, but, it is curious, even quite recently the Germans, who cannot be ignorant of Vedeler's papers, speak as if the facts were still debatable, or as if they had appeared abroad, instead of in the principal place of resort for such papers, in the whole German current literature. I am inclined to think that in matters of science Englishmen live less on an island than other people. I trust that in the above remarks I shall not be interpreted as questioning the good faith of the theorists, but only as criticising their scientific methods. It is true that no theory can be developed without the aid of the imagination, but all ideas should be kept in limbo until they have been subjected to the severest criticism by their parents. It is also true that, what is true is generally simple. But the converse—what is simple is generally true—by no means holds; and the mechanical theory seems to be an instance of such logic."

In the able Lectures from which the foregoing observations are taken we have, I think, some evidence that Dr. Champneys' own logic is not altogether free from such fallacies as he ascribes to the supporters of the mechanical theory of dysmenorrhœa. However, without attempting to discuss that question, I may at least, in reply, state that in many hundreds of instances patients labouring under spasmodic dysmenorrhœal suffering have come under my care in hospital and private practice in whom, on careful examination, no other cause could be assigned for the symptoms complained of than the existence of stenosis of the cervical canal or its constriction by some flexion. And in such cases the operative treatment of the stenosis or the rectification of the flexion, without any other treatment, was followed by the complete and, as far as I know, permanent cure of the dysmenorrhœa. Therefore, unless Dr. Champneys, or anyone else, can offer a more logical explanation of these facts than that to which my possibly illogical mind would lead me—viz., that in those cases the cause of the patient's monthly spasmodic suffering was obstruction, and its cure was effected by the mechanical measures I employed—I shall adhere to my own opinions and practice, and I would venture to recommend other practitioners to act on similar views, until at least they obtain some proof (which it may, perhaps, be found difficult to afford) that the doctrines of those who teach otherwise are founded on larger and more accurate clinical observation or more successful results, than have been obtained from the methods of treatment heretofore adopted by myself and others in such cases. Relying on that experience, therefore, I shall now endeavour to put my views on this subject briefly before you.

Diagnosis.—Of the uterine causes of dysmenorrhœa, as well as of the commonly co-existing sterility of the patient in such cases, probably the most important, as well as the most

curable, is obstruction, congenital or acquired, of the cervical canal. The consequences, immediate or remote, of this condition are daily brought under gynæcological observation, and unquestionably few of the complaints that come before us in our branch of practice give rise to more intense and persistently recurring suffering. Nor is there any disease which, when unrecognised as to the true cause, and hence, as is too often the case, treated by sedatives, spasmodics, and other equally ineffectual palliative remedies, may produce more serious ill effects, not merely on the general health, but also on the cerebro-nervous system of the patient. Hence the necessity in all cases of persistent uterine dysmenorrhœa (when not obviously of a purely nervous form) of careful local examination and the use of the sound, by which the existence, position, and cause of any uterine mechanical obstruction to the menstrual flux can be ascertained.

Causes.—Obstructive dysmenorrhœa may result — 1st. From stenosis, congenital or acquired, of the cervical canal, and more especially of the os internum. 2nd. From constriction of the canal by an acute flexion. 3rd. From the presence of a uterine tumour. 4th. From obstruction occasioned by an inter-cervical neoplasm. In the majority of instances, however, this condition is consequent on simple congenital atresia of the cervical canal, or on its occlusion by plastic exudations resulting from endometritis. The next most commonly observed of these mechanical causes of dysmenorrhœa are the different flexions of the uterus. The frequency of the former class of cases is far greater than is generally supposed. Thus, as I stated in a paper read at the Berlin meeting of the International Medical Congress, in my hospital nearly 11 per cent. of instances of obstructive dysmenorrhœa or of sterility similarly caused had been under observation in a total of 7,000 gynæcological cases.

Symptoms.—I need not here enlarge on the symptoms of obstructive dysmenorrhœa, as these vary widely in each case in accordance with the situation, causes, and extent of the obstruction and the constitutional condition of the patient. The existence of obstruction, whether from stenosis, flexion, or pressure, may be commonly inferred from the character of the pains attending the uterine efforts to overcome the mechanical obstacle to the catamenial flux, these pains in such cases being extremely similar to the grinding or cutting pains of the first stage of labour, and often equalling, if not exceeding in intensity, the throes of parturition rendered difficult by persistent rigidity of the os uteri. Moreover, under such circumstances, we are very likely to have extension of the resulting irritation from the uterus to the Fallopian tubes or to the ovaries, producing all the symptoms of salpingitis or oöphoritis. After a variable period of suffering, in most instances, the obstruction is at last temporarily overcome, and the dysmenorrhœal dribble is eventually succeeded by a profuse menorrhagic discharge, after which the patient's troubles are over for that month. In some instances the obstruction proves more intractable, and the pain continues until the catamenial flux has ceased, or the uterine action, as occurs in some instances, forces the retained menstrual fluid through the free orifices of the Fallopian ducts, and so may give rise to pelvic peritonitis or hæmatocele.

Treatment.—In any case of obstructive uterine dysmenorrhœa it is obvious that the only rational course of treatment is the removal of whatever may be the physical impediment to the escape of the menstrual fluid, and that in such cases no curative effect can be expected from any drugs, however useful some of those may be as palliative agents. Thus, for instance, if catamenial pain be due to an acute flexion, the dysmenorrhœa can be cured only by rectification of the malposition; or if occasioned by pressure on the cervix of a

uterine or other tumour, this must be removed or pushed well up above the pelvic brim. In like manner in those far more numerous cases in which the difficulty arises from cervical stenosis, this must be overcome before we can hope to remove the menstrual suffering.

Dilation of the Cervical Canal.—I do not propose in this connection to rediscuss the various means from time to time suggested for the mechanical dilation of the cervical canal in cases of obstructive dysmenorrhœa; nor need I here refer again to the history of the successive steps (of which an account may be found in my previous communication to the Academy on this subject) by which the formerly-employed methods of expanding the stenosed passage by means of sponge tents, laminaria bougies, and other similarly tedious, painful, and often hazardous appliances, have now become generally replaced by rapid dilation with the assistance of one or other of the instruments devised for that purpose by Lawson Tait, Hegar, Duke, and others, including myself.

With regard to bi-bladed expanding instruments of this kind, such as Duke's or mine, Dr. Champneys says, "Two or three-bladed dilators are bad instruments; they stretch the cervical canal unequally and are liable to tear it. They are out of fashion in England, though not apparently in America or on the Continent." The first of these assertions is merely a statement of Dr. Champneys' individual opinion, which, however emphatically expressed, is not, as far as I am aware, entitled to any greater consideration than he apparently accords to the experience to the contrary of those who, like myself, have largely and advantageously used the instruments of which he disapproves. Whilst, with regard to the second point, it seems obvious that if, as Dr. Champneys has persuaded himself, dilators of the class in question were actually "out of fashion in

England," there should be little, if any, demand for them, inasmuch as a taste for acquiring gynæcological instruments is one not generally indulged in save by those likely to use such appliances. Therefore, as I am sure that Dr. Champneys will be glad to have any error of his, however trivial, corrected, I may observe that I have very recently been informed by one of the principal London surgical instrument manufacturers—viz., Messrs. Arnold & Son—that they "have sold some hundreds of these dilators since their introduction." Hence, two-bladed cervical dilators can hardly be regarded as out of fashion in England, and, I may add, probably never will be so in any country as long as women continue subject to obstructive dysmenorrhœa and are judiciously treated by cervical dilation for that complaint. Be that as it may, however, at any rate I still prefer the dilator I introduced here some years ago, and which has been subsequently modified and thus rendered more effective for its purpose. This instrument, the latest pattern of which I now exhibit, differs from others of the same class in the circumstance that the expansion effected therewith is in imitation of the natural process of uterine dilatation during menstruation—viz., from the os internum downwards in the course of the cervical canal—and, from subsequent experience of its use in many hundreds of instances of obstructive dysmenorrhœa, I would recommend it to other gynæcologists as an effective and, when properly used with due caution and in suitable cases, a safe means of rapidly overcoming cervical stenosis, and so relieving consequent dysmenorrhœal pain.

The Use of Metrotome, &c.—In such cases I regard the use of any cutting instrument as, generally speaking, unnecessary, occasionally giving rise to troublesome hæmorrhage, and still more objectionable by reason of the liability of the cervical incision to become a portal for the intro-

duction of sepsis, as I have seen exemplified in more than one instance. In some few exceptional cases of dysmenorrhoeal stenosis, however, the obstruction may be so unyielding that it cannot be overcome by any available method of dilatation, and such cases may probably be best dealt with by the cautious use, with due aseptic precautions during and after the operation, of Sims' knife, or else of a single-bladed metrotome, such as Simpson's, preferably to any of the more recent instruments of the same kind. Whilst as to scissors such as Keuchenmeister's, the use of which is still recommended by some writers in the cases now under consideration, all I can say is that their employment appears to me likely to expose the patient to all the septic dangers above referred to with little probability of relieving the disease for which they are employed, cutting, as they do, the os externum, where obstruction comparatively seldom exists, into a gaping wound with everted or retracted lips, and leaving untouched the os internum, which, as I believe, is the actual seat of the stenosis in nine-tenths of these cases.

Maintaining Patency of Canal.—Whatever method of dilatation be adopted to effect the cure of obstructive dysmenorrhœa, it is obviously essential that the patulous condition of the cervical canal should be permanently maintained. For this purpose, as far as I know, no very generally applicable and effective means was available, with the exception of Dr. Greenhalgh's elastic stem, which I have used with advantage in many instances, until a couple of years ago, when Dr. Duke, of Dublin, suggested the use of a spiral wire stem, to be introduced directly after operation and kept *in situ* until all tendency to subsequent contraction has been overcome. This suggestion was one which at first I was rather sceptical as to the advantage of, as it appeared to me probable that the

stem might prove not only an immediate source of local irritation, but also subsequently a focus for sepsis. However, from a fair trial of this appliance, which I have now used very extensively, I can corroborate the advantages claimed for its employment. These stems, which are made by Messrs. Arnold, being extremely flexible and well open from point to point, may be left *in situ* for an almost indefinite period, provided always that due aseptic precautions be taken by regular daily vaginal irrigations to keep the passage and stem perfectly free from all morbid secretions. As a rule, however, I think that no stem should be worn continuously for more than a month; and in every case if, notwithstanding the precautions above referred to, any inflammatory trouble or septicæmic symptoms should supervene, it should, of course, be immediately withdrawn.

Congestive Dysmenorrhœa is of special clinical interest as a frequent result of chronic endometritis, and hence is more commonly met with later in life and in married women than other varieties of dysmenorrhœa. In the congestive condition on which the form of difficult menstruation is consequent, besides the uterus and its appendages, the other pelvic as well as abdominal viscera are usually more or less involved, as is more particularly seen in the frequently engorged state of the portal circulation in such cases. In most of these instances the tumefied uterus is, moreover, to some extent displaced, either by ante- or retroflexion or version from the greater degree of congestive hypertrophy that may be developed in one or other of its walls, or else is forced down lower than its normal position in the pelvis by its generally augmented bulk and weight. In the same way the functions of the rectum and bladder are interfered with, a certain amount of cystitis or vesical irritability or hæmorrhoidal trouble being seldom absent in cases of this kind.

Symptoms of Congestive Dysmenorrhœa.—For a few days before the menstrual period the patient generally complains of some degree of lumbar pain and sense of uterine weight and bearing down, gradually developing on the approach of the “change” into acute localised intermittent pain and persistent feeling of distension and discomfort in the hypogastric region, which steadily increases, and is followed by distinctly expulsive pains, accompanied by a scanty menstrual discharge, in many instances intermixed with membranous shreds or clots. This often abates or entirely ceases on the third or fourth day, and in some cases the discharge ceases for a day or so during the menstrual period, and then returns in intermitting gushes.

Treatment.—To cure congestive dysmenorrhœa we must, in the first instance, cure the abnormal uterine condition or hyperæmia of which the menstrual difficulty is symptomatic by those measures that have been fully described in my previous communication on endometritis. In all cases such as these, warm hip-baths and hot-water irrigations (per rectum and vaginam) are generally serviceable in the immediate relief of the catamenial pain, as in the same way are also the various time-honoured and well-known special uterine and general nerve sedatives and stimulants, such as castor, guaiacum, &c., to the use of which I need not here further allude. In the general treatment of congestive dysmenorrhœa it is of special importance to relieve abdominal and pelvic visceral congestion as far as possible by saline purgatives, such as a teaspoonful of Carlsbad or Glauber’s salts in a half tumblerful of hot water once or twice a day; and with the same object of diminishing as far as possible the hyperæmia of these and other organs, nothing appears to me more generally serviceable in such cases than the free use of bichloride of mercury with iodide of potassium and bark.

In such cases local treatment must always be conjoined with the general remedies just referred to. In many instances, to unlock the uterine hyperæmia, free scarification of the cervix, followed by the glycerine tampon and hot-water irrigations, if persevered in for some time will prove effectual, and should be fairly tried in the first instance. In some cases, however, it will also be found necessary to resort to intra-uterine treatment by the dilatation of the cervical canal and the subsequent employment of the curette, by the use of which the diseased endometrium may be scraped away, whilst by the ensuing hæmorrhagic discharge the local congestion may most directly be dealt with.

Probably no one who has read my previous communication on the subject of obstructive dysmenorrhœa will accuse me of any tendency to minimise the importance of mechanical and operative treatment in such cases. But, at the same time, I am bound to observe that these methods are frequently pushed to an undue extent by practitioners who apparently ignore the fact that, common as is the obviously obstructive form of dysmenorrhœa, this complaint may also occur in cases and from causes beyond direct or mechanical treatment of this kind—viz., as the result of ovarian or Fallopian tube disease, or as the consequence of a constitutional, neurotic, or hyperæsthetic condition, which may be best treated by attention to the general health of the patient. In the latter cases the most signal benefits are often derivable from the free use of nerve tonics, such as the preparations of valerian and its combinations, and more especially the valerianates of quinine and iron, together with bromides of sodium or potassium as nerve sedatives; and, if necessary, as a temporary measure, to allay extreme dysmenorrhœal pain, opiates and cannabis indica, which, in such cases, may be

most advantageously given by the rectum rather than by the mouth or hypodermically. This fact is one of such practical importance, and is so generally neglected at the present day, that I may here cite the forcible terms in which it was stated in a paper "On the right use of Sedatives in Diseases of the Womb," published in the *Lancet* many years ago by Dr. Tilt, whose views on that subject are identical with my own experience. "He must indeed be young in practice," says the writer just named, "who is not aware how frequently pain and neuralgia, under various forms, appear as an important symptom of morbid menstruation, of diseases at the change of life, and of uterine and ovarian affections. Neuralgia in connection with the reproductive system is still more frequent in women of the upper classes, in whom a nervous temperament has been inherited and rendered more intense by injudicious schooling and by habits at variance with correct notions of hygiene. Suppose a patient suffering habitually from nervous uterine irritability, from bearing-down pains, vesical tenesmus, and severe pains in the sacrum and thighs, the necessity for sedatives will strike everyone. Most medical men will give them by the mouth, either in such moderate doses that the patient's sufferings will be long in abating, or, should the quantity have been sufficient to assuage the pain, the drugs may have acted so strongly on the system that it would take some days to recover from the poisonous effects. Afraid of this, others would apply sedatives to the pelvic regions or loins; but before relief could be given much time must elapse, though, if the fomentations had been associated with sedative injections into the rectum, relief would soon be afforded, without any subsequent ill effects, because the remedy being applied directly to the diseased nerve the dose could be proportioned to the intensity of the pain. Is it not, then, better

to give sedatives by the rectum than to have a patient in a habitual state of suffering, or with the occasional variation of an overdose of opium? This is applying the remedy in the right place, or applying the sedative as near as possible to the suffering nerves."

Nervous or Spasmodic Dysmenorrhœa.—In early menstrual life probably the most frequent form of dysmenorrhœa is that described as neurotic or spasmodic, and, moreover, in every variety of this complaint, and at every age at which it may occur, the neurotic element to a large extent complicates whatever other cause painful menstruation may be dependent on; and hence the brief account which I have already given of the ordinary symptoms and general palliative treatment of dysmenorrhœal pain may be also applied to the nervous or neuralgic types of this affection.

In speaking of spasmodic dysmenorrhœa, Dr. Champneys institutes an analogy between the phenomena of asthma and the vaso-motor disturbances in the parts concerned in the causation of painful menstruation, which, in reference to structures and functions so widely dissimilar as those of the pulmonary and utero-ovarian organs, seems to me wholly untenable. The same writer makes another statement, viz.—"For if, as we have seen, the smallest cervical canal can transmit the menstrual fluid without sensible hindrance, much more can it admit the spermatozoa." In this little "if," however, lies the pith of this problem, for, in the first place, I for one deny that the smallest cervical canal can transmit the menstrual fluid without sensible hindrance, and, secondly, the comparison made between the viscid, or often shreddy or clotted, and *débris*-laden menstrual flux and the uniform opalescent and lubricating or semi-oleaginous seminal secretion can hardly be taken seriously. "It must be remembered," he further observes, "that uterine colic is not associated with spasmodic con-

traction of the uterus, though it may be associated with a condition of tetanus without narrowing of the canal as in after-pains. The great remedy is pregnancy and parturition."

If pregnancy and parturition, however, are not sufficient to ensure the multiparous patient against the common occurrence of after-pains, it may be fairly asked how, in the name of common sense, can they remedy a tetanic condition of the uterus which is referred to as analogous to that giving rise to those after-pains? Admitting, however, that parturition at full term may cure dysmenorrhœa, I cannot believe in the cure of dysmenorrhœa by impregnation unless continuing to full term, as I have observed that patients under such conditions who may have become pregnant are particularly liable to abort at what would have been a menstrual period had impregnation not occurred, and that the subsequent dysmenorrhœal troubles were thereby but intensified.

Membranous or Pseudo-membranous Dysmenorrhœa, to which attention was first called by Morgagni, who, upwards of a century ago, observed the periodical expulsion of membranous casts or shreds with the catamenial flux in cases of painful menstruation, has been and is yet very generally described as a distinct and special form of the complaint under consideration. This view appears to be quite erroneous, however—the common occurrence of such shreds or clots in the menstrual discharge of dysmenorrhœal patients having been clearly demonstrated by Dr. John Williams in his conclusive statistics. Moreover, as was long since shown by Oldham and again by Bernutz, whose remarks as bearing on this point may be here briefly recapitulated, these products, which are composed entirely of the histological elements of the uterine mucous membrane and ought consequently to be ascribed to a distur-

bance of the ordinary physiological moulting of which the generative organs become the seat at each catamenial period, are occasionally met with in typical instances of each one of the three forms of dysmenorrhœa—*i.e.*, obstructive, congestive, and neuralgic, as already described. Nor is the membranous expulsion of such importance as a cause of painful menstruation as is commonly supposed. On the contrary, as Bernutz observed, "The difficulty arises from the morbid condition of the attached uterine mucous membrane, especially the cervico-uterine portion of it." "I willingly allow," adds Bernutz, "that the dysmenorrhœal membrane floating about the cavity of the uterus may occasion temporary difficulty to the exit of the menstrual fluid (acting as a ball valve), just as a clot might do, but I do not believe it would give rise to so serious an obstruction as occurred in these cases, unless there co-existed defective dilatation of the cervico-uterine canal." This view has not been displaced by any more recent observations, and in my own practice I have acted on it with advantage by dilating the cervical canal and curetting the endometrium in many instances of this kind.

DR. ATTHILL, though he agreed with Dr. More Madden on some points, could not do so in others. He believed that flexions of the uterus very rarely, if, indeed, ever, were *per se* a cause of painful menstruation; but if endometritis were present in addition to the flexions or a conical cervix and pin-hole os, then the function would probably be painful. As to mechanical obstruction as a cause of dysmenorrhœa, that was exaggerated. You met with cases frequently in which the os uteri was very small, and yet the function was painless for years, and then became painful. Sexual intercourse, in such cases, tending as it did to cause an increased determination of blood to the organs of generation, was sometimes the exciting cause of dysmenorrhœa in such cases. Cold was another inducing cause of congestion and subsequent inflammation of the endometrium. As to treatment in inflammatory cases,

division of the cervix or dilatation were, if practised, only preliminary steps to such treatment as would remove or relieve the local disease, and neither should be practised unless the stenosis were such as to prevent the free exit of the menstrual flow and of the mucous discharge which always was present in these cases; but, if the cervical canal were too narrow and the os too small to permit of this, dilatation or division was justified. It should be borne in mind that dilatation was only a temporary proceeding, and that within a few days after it had ceased to be practised the os and canal would be as small as ever; therefore, if pregnancy did not occur immediately no permanent benefit followed, and it was frequently better to divide the cervix, taking care to prevent the incisions healing up, but neither procedure alone would suffice. The endometrium must subsequently be treated. Dr. Atthill objected to the use of any form of metrotome, and always used a scissors for the purpose of dividing the cervix. No doubt, in some cases the pain was due to spasmodic contractions of the muscular fibres of the uterus, which dilatation might relieve, but seldom cured. In a case recently under his care, in which amenorrhœa had existed for months, pain at tolerably regular periods and of an excruciating character was experienced. Benefit in this case resulted after dilatation had been regularly practised at short intervals for some weeks.

DR. MORE MADDEN replied.

ON DILATATION OF THE UTERUS, AND THE TREATMENT OF SOME OF THE COMMONER FORMS OF UTERINE DISEASE.

BY LOMBE ATTHILL, M.D.

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[Read in the Section of Obstetrics, April 29, 1892.]

THE wonderful advance which has been made in abdominal surgery within the last few years—an advance which in no small degree has been due to the efforts of gynæcologists—while conferring the greatest benefits on mankind, has had this disadvantage, that in a very appreciable degree it has diverted the attention of students, and, indeed, that of many practitioners, from the study of those ailments which are of common occurrence amongst women, and are the cause of so much distress, and directed it too exclusively to those which are curable by surgical means only. Abdominal surgery is most fascinating, and it is easy to understand how pupils rush to see the skilful operator perform an abdominal section, and practitioners endeavour to imitate him; but after all, it is granted to but a comparatively few to have the opportunity of performing these often most difficult operations, while, no matter whether specialists or engaged in general practice, all will constantly be called on to treat those other numerous ailments which are peculiar to women, and which, although they may be looked upon as of less importance, are often the cause of the greatest misery to the patient.

When I commenced my professional career, gynæcology as it is now understood did not exist; and even when first attached to the Rotunda Hospital as assistant to the Master,

I saw nothing of uterine disease, and it was not till the teachings of Sir James Simpson became generally known that any systematic attempt to study and teach the treatment of those diseases which are peculiar to women was attempted, not alone in this city, but throughout the whole kingdom. To him mankind are deeply indebted; but for him the general use of anæsthetics would have been delayed for years; and it was he who prominently brought forward the practice of dilating the uterus, and taught us that its interior could with safety be thoroughly explored.

Dilatation introduced with the object of facilitating the removal of polypi was soon practised with view of diagnosing and treating other forms of intra-uterine disease, and was in the first instance effected by means of prepared sponge tents, which were objectionable, because it was necessary first to introduce a small one, and after it had expanded a larger one, and then a third or a fourth, before the requisite degree of dilatation was effected—a very tedious process, which moreover entailed danger of septic poisoning from the nature of the substance used; and besides, the dilatation was frequently unsatisfactory because while the os externum was widely distended, the os internum would often be hardly so at all.

Compressed laminaria tents were then employed, at first in the same manner as those made of sponge, one only being introduced at a time, till the method of inserting several at once was introduced by Dr. G. H. Kidd. This was a great improvement, and if it be carried out with the necessary precautions, is quite safe.

Subsequently the method of dilating the cervix rapidly, by means of graduated bougies, was practised, and was advocated with his usual ardour by the late Dr. Matthews Duncan. It is a method admirably suited to certain cases, but, without doubt, not to all; and it has therefore never entirely recommended itself to me. Those who care to do so will find

Dr. Duncan's paper on the subject in the second volume of the *British Medical Journal* for 1872, and the subsequent discussion, in which I took part; and, while a more extended experience has in some degree modified the views therein expressed, still I have not altered my opinion that, in not a few cases, rapid dilatation of the cervix is not a judicious proceeding, and that sometimes it is a dangerous one.

Dilatation of the cervix—not alone during parturition, but sometimes also under other conditions—is a natural process, and, when conducted artificially, Nature should as far as possible be imitated.

If anyone will carefully watch a case of purely natural labour from its very onset, he will easily realise that the opening of the mouth of the womb is not effected, so far certainly as its initial stage is concerned, by the forcible pressure of its contents on the os uteri. The cervix softens and relaxes previous to the commencement of labour, and the os not infrequently expands, sometimes days before uterine action sets in, to such an extent as to permit the finger to touch the membranes. Again, when premature labour is induced by the introduction of a catheter, I have observed the os to expand to a considerable extent before any attempt at uterine action was excited, and I am perfectly convinced that the initial steps of labour in general are, first relaxation of the muscular fibres surrounding the os, and then their retraction, and it is subsequent to this that the contraction of the body of the uterus comes into play.

When we desire to dilate the cervix by artificial means it is most desirable to induce this relaxation and tendency to retraction. This cannot be done rapidly; if, therefore, dilatation be accomplished by means of bougies alone, no matter what their shape or form may be, nature's process of effecting that object is ignored.

Under certain circumstances this may be a matter of no

importance, and if the patient has had a number of children and be one in whom the cervical canal is patulous, or if from any cause, such as imperfect involution subsequent to labour or abortion, the cervix is soft and dilatable, rapid dilatation is justified, and may safely be practised; but in women who have never conceived, or in whom, notwithstanding that parturition has occurred, the os is small or the cervix rigid, it should not in my opinion be attempted.

In all such cases I introduce one or two laminaria tents, or short lengths cut off a laminaria bougie. I never now pack the cervical canal with these as I used to do in former days, for my object is not forcibly to dilate the canal by their expansion, but to soften its structure and set up the tendency to retract I have referred to, and I find this is best effected by the introduction of only one or two pieces of laminaria, which are not to be tightly wedged into the canal. The vagina should be thoroughly disinfected by the free use of a solution of carbolic acid or of the perchloride of mercury before this is done, and if the cervical canal will admit of the introduction of a small-sized Bozman's tube, the cavity of the uterus also should be washed out with it.

At the end of eight or ten hours the desired effect is produced; the cervix, on the withdrawal of the pieces of laminaria, will be found so softened, and its tissue so relaxed, that in general No. 10 or 12 of Hegar's dilators pass through the os internum readily. Sometimes, indeed, a much higher one will do so, while to dilate the cervical canal so as to permit No. 10 or 12 of Hegar's dilators to pass through the os internum in a case in which the cervix has retained its ordinary degree of rigidity, without the previous use of the laminaria, is a very tedious task, unless, indeed, the operator does not hesitate to run the risk of rupturing some of the muscular fibres of which its tissue is so largely composed, and very possibly also of lacerating the lips of the os, which

must be held by a vulsellum, and as this proceeding is painful as well as slow, an anæsthetic must be used, and its influence maintained all the time.

If, however, the patient has, from any cause, a soft and relaxed cervix and patulous canal, specially if she have been recently delivered at the full term of pregnancy or aborted, the previous use of laminaria may be unnecessary, and dilatation by means of Hegar's or some other of the dilators which are in use, effected, without any preparation further than the thorough disinfecting of the uterus and vagina. Once No. 12 of Hegar's dilators passes the os internum, a small curette can be used, but it is wiser to continue the process till No. 15 passes into the uterus, for then a larger curette can be introduced, and the results will be more satisfactory; but if it be deemed necessary to explore the cavity with the finger, the dilatation must be proceeded with till No. 20, or possibly No. 25, pass freely through the os internum.

It is often wise, in dilating the uterus, specially if any difficulty is experienced in the introduction of the next size dilator, to reintroduce the one just withdrawn, first having washed and disinfected it thoroughly, and to leave it *in situ* for a little time, or even withdraw and introduce it a third time. It is surprising how this seems to lessen the resistance of the muscular tissue to the action of the next sized dilator, and this strengthens the opinion that the opening of the os internum is not by any means altogether due to the mechanical action of the instrument used.

In the early years of my practice I was much opposed to the use of the curette, and termed it in one of my lectures "an unscientific instrument," and, doubtless it is so, and I objected to its being used unless the finger were first introduced, and the part to which it should be applied by this means ascertained; but I am now prepared to retract this

expression of opinion so far as to say that, in the majority of cases, it may be employed without dilating to the extent needed for this purpose; but it is much more satisfactory to have explored the condition of the cavity, and I have more than once regretted that I had omitted to do so. Still, the temptation to shorten the proceeding, and with it to lessen the chance of injuring the patient, often induces us to use the curette at as early a stage as possible. The curette used should not, however, be too small—to use such a one is a great mistake; these miniature curettes are nearly useless, and the facility with which they can be employed induces some practitioners to use them freely, sometimes in the study, and even without the patient's knowledge. This certainly is wrong, and it is calculated to injure the patient as well as to bring discredit on a very useful instrument.

But the use of the curette alone should not be relied on. It has to be used to a great extent by guess; at one moment we find that we remove with it perhaps a large portion of thickened and unhealthy mucous membrane, and the next time we withdraw it we find we have removed nothing but some shreds of healthy tissue, so that it is not alone possible but probable that portions of the unhealthy surface may escape the action of the instrument; I therefore always adopt further treatment. Formerly I applied the fuming nitric acid at once, and often with marked benefit, and never—since I introduced the use of the vulcanite cannula to protect the cervical canal—with any disagreeable results. But I now very seldom adopt that treatment, because, if used immediately after curetting at the time when the acid is easy of application, the bleeding from the surface of the cavity is generally so free that it neutralises the action of the acid; and if used some days subsequently, you have generally to pass the probe which carries up the cotton saturated with the acid through so small a cannula that only spots of the surface of the

endometrium are cauterised. I therefore now almost invariably inject into it, after the lapse of about three days, a small quantity of iodised phenol, using for this purpose one of Baum's syringes. As I have for the last ten years adopted this method with very satisfactory results, I can speak of it with confidence.

And here again I have to recant an opinion I expressed in some of my former writings. If I formerly objected to the curette while now I advocate its use, I still more strongly objected to the injection of fluids into the cavity of the uterus. Now, as the result of careful observation and lengthened experience, I practice this very treatment which I formerly condemned. Indeed, few days elapse in which I do not inject into the endometrium some tincture of iodine or iodised phenol, or wash it out with a weak solution of carbolic acid, hazeline, boracic acid, or some other fluid, with the best results. The quantity I inject at first varies from 5 minims to 30, increasing it to a drachm if it be well borne. But this practice cannot be rashly undertaken or be unskillfully carried out without risk to the patient. In careful hands, however, it is quite safe.

The cause of this change of opinion on my part is easily understood. My objection to the practice was theoretical. I now advocate the practice which formerly I condemned because I have proved it to be safe as well as efficacious; but to carry on the treatment safely two things are essential—First, that the os uteri and the whole length of the cervical canal be sufficiently patulous, so that the fluid injected can escape freely and without delay; secondly, that the quantity of fluid injected be small in quantity, especially on the first occasion. Unless the cervical canal be abnormally patulous, about 5 minims only should be at first injected, the quantity to be subsequently gradually increased if it is found to be well borne, but a drachm is the maximum I ever inject of any

fluid, and seldom, indeed, more than 15 or 20 minims. The larger the quantity the greater is the danger of its causing trouble, and the quantity much more than the quality of the fluid injected is to be considered. I have met with cases in which very severe pain has followed the washing out of the uterus with a weak solution of carbolic acid, and even with tepid water, when I had injected a larger quantity of the fluid than I had previously done, while no pain of any importance was experienced by the same patient when a small quantity of strong iodised phenol had been used.

The first patient on whom I practised this treatment was a lady, who, having passed the climacteric period, was attacked with profuse hæmorrhage; I dilated the uterus and removed a mass of soft shreddy membrane, of which a portion seemed to have been hanging into the cavity. I feared that it was malignant, and had it examined with the microscope, but nothing definite could be detected. This lady rapidly improved, was quite well for six months, then the hæmorrhage returned more profusely than before—indeed it was on this occasion so alarming that she was almost pulseless before it could be restrained by plugging. I again dilated, and now found the uterus filled with a soft pulpy mass undoubtedly malignant. Were I to meet with such a case now I would extirpate the whole organ, as I have since done with success, but abdominal surgery was not then in the advanced state it now is, so I contented myself with carefully removing with the curette every portion of the diseased surface, and subsequently injecting about 15 or 20 minims of iodised phenol once a week regularly. I was forced to do this, for the hæmorrhage soon showed signs of recurring, but the injections kept it in abeyance. I continued this treatment for upwards of two years, and though the patient slowly lost ground and finally died, the hæmorrhage never once recurred to an alarming degree, and I

have no doubt but that life was prolonged for a year or more in consequence of this treatment. But it is not of its use in such cases that I am now speaking; in them hysterectomy should be performed, but its good effects in this one encouraged me to try it in others. And I soon found it applicable to a variety of cases.

The fluid I usually employ is the iodised phenol as recommended by the late Dr. Batty. It is made by dissolving one part of pure iodine in two parts of carbolic acid by the aid of a gentle heat; a small quantity of methylated spirits should then be added to keep it sufficiently thin for use. The effect of this when injected into the cavity is to cause the surface with which it comes in contact to shrivel up, and in a day or two to peel off—in fact it acts primarily as a mild caustic; some of the iodine, no doubt, is also absorbed, and many patients complain of the taste of the iodine in the mouth. In cases, therefore, in which there is reason to believe the lining membrane of the uterus to be unhealthy, and where the symptoms are not sufficiently grave to induce us to decide on dilatation and curetting, the injection of the iodised phenol is indicated. I have also on several occasions employed it when patients for some cause could not or would not submit to curetting, and in a considerable number of them found it effect a cure, or at least be productive of marked benefit.

In two cases in which menstruation continued to be so profuse some months subsequent to abortion at an early period of pregnancy, and the uterus remained so large and soft as to lead to the belief that a portion of the membranes might be retained, the injection has been followed by the expulsion of a mass which I believe to have been the remains of the ovum shrivelled up by and then expelled, in consequence of the action of the phenol. In both these cases recovery was perfect without further treatment.

In all cases in which I dilate and use the curette I inject the iodised phenol several times, at intervals of from four to six days according to the nature of the case, commencing on the third or fourth day after the operation. I have several times been consulted by patients who have been curetted without deriving permanent benefit therefrom, and I believe that this has been in general due to neglecting to adopt this practice or some such after-treatment. I have generally found, if the interval since the operation has not been very long, that the injections of the iodised phenol succeed in effecting a cure.

The number and frequency of the injections must vary with the nature of each case, and therefore must be decided by the practitioner at the time, but it is necessary to bear in mind the fact that the first, and possibly the second, injection is often followed after the lapse of a few hours by some bleeding. This is specially the case if the curette has not been previously used, and it is probably due to the action of the phenol causing the superficial layer of mucous membrane to peel off rapidly, leaving a vascular surface exposed, which bleeds sometimes freely. This bleeding is of no importance, but sometimes alarms the patient, and she should be told that it may occur. If it continue after the injections have been repeated three or four times, it generally indicates that patches of large and vascular granulations exist, which, if the curette has been used, have escaped its action, and, whether it has been used or not, proves that the further use of intra-uterine injections will be useless.

I am far from wishing it to be understood that I deem this treatment applicable to even the majority of cases of disease of the uterine cavity, but I believe that it will frequently render the use of the curette unnecessary, and that, if not always essential, it is so in the majority, and useful in all those in which curetting has been practised, while in cases

where uterine catarrh is present great benefit will often be derived from the practice.

DR. C. YELVERTON PEARSON (Cork) said he rose with a considerable degree of hesitation to speak on a paper by Dr. Lombe Atthill, as, although not a pupil of Dr. Atthill's, still he owed many of his earlier impressions on women's diseases, and not a few of his earliest successes in gynæcological practice, to the teaching contained in Dr. Atthill's writings. From the paper they had just listened to, it was evident that Dr. Atthill's opinions had undergone a considerable revolution during the past 10 or 12 years; and it was not to be wondered at that his (Dr. Pearson's) opinions had also changed considerably during that period. It was indeed gratifying to him to find his present views on the treatment of intra-uterine disease so closely in accord on most points with those he had just heard expressed. The fact that there were so many methods recommended for dilating the cervix uteri and such a number of dilators in existence, which number was being constantly augmented—was, to his mind, a sufficient indication that no one method of dilatation was satisfactory in all cases. His own practice varied according to the nature of the case. For example, if a case presented itself where there was no doubt that the symptoms were due to an unhealthy condition of the endometrium he employed rapid dilatation, and curetted the interior of the uterus at once, unless he found considerable resistance at the os internum, in which case he did not think it justifiable to proceed, but inserted one or more laminaria tents, as advocated by Dr. Atthill, and waited for six or more hours until they had produced their wonderfully softening influence, after which the dilatation could be readily carried out to the necessary extent. In all cases where there was an unusually narrow cervix he considered the combined method the safer. Again, where for purposes of diagnosis it was considered necessary to explore the interior of the uterus with the finger for polypus, submucous fibroid, &c., he considered it advisable to carry out a portion of the dilatation by means of tents, which he believed, with Dr. Atthill, were quite safe if carefully used, without tight pushing, and with due regard to cleanliness. It had been his custom until somewhat recently to apply either iodised phenol, or a saturated solution of carbolic acid

in glycerine after employing the curette. But of late he had ceased doing so, and merely douched out the uterus with creolin and hot water, or the latter alone. He had no experience of the use of intra-uterine injections in the manner indicated by Dr. Atthill; in the first place, he was rather afraid of them, which was probably the result of prejudice; and, in the second, the curette had yielded him such satisfactory results that up to the present he had felt no inclination to try other methods. But possibly the strong way in which injections had been advocated by Dr. Atthill might cause him to alter his practice in future. The dilators he employed were not any of those in general use, and he hoped on some future occasion to bring his own dilators under the notice of the Section.

DR. SMYLY also spoke, and DR. ATTHILL replied.

ON A CASE OF DOUBLE MONSTER.

BY W. J. SMYLY, M.D., F.R.C.P.;

Master, Rotunda Lying-in Hospital.

[Read in the Section of Obstetrics, April 19, 1892.]

THE mother was a tripara aged twenty-seven. Four feet presented; it was supposed to be twins. Labour occupied 21 hours, and was easily terminated by traction on the legs.

The deformity is different from any that I have seen or read of. The right sides of both heads are fused together, so that a face appears on both aspects, with an occiput and ear on either side of it; one face is further deformed by a harelip. The bodies are fused together in front as low as the umbilicus (Thoracopagus), but the eight extremities are perfect. There was only one umbilical cord and one placenta. Both children were males.

1. These double monsters may be classified into those united by their heads, *duplicitus posterior*.

2. Those united by their caudal extremities, *duplicitus anterior*.

3. Those united by their bodies, *duplicitus parallela*.

Of the latter there are two sub-varieties—*a*. Double-headed, with united bodies. *b*. Heads united but bodies separate below. The specimen before you is an example of the latter form, which, according to Dr. Playfair, is the rarest of all. Two cases only are referred to in his monograph on this subject.

SECTION OF PATHOLOGY.

MULTIPLE ABSCESES OF THE BRAIN.

BY ALFRED R. PARSONS, M.B. UNIV. DUBL.;

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[Read in the Section of Pathology, Friday, January 22, 1892.]

A COMPLETE and accurate diagnosis of intracranial disease is often beset with difficulties. It demands that we should be able not only to state the site of the lesion, but also to describe its nature. Guided by what experimental research and pathological investigation have taught us of the functions most intimately connected with certain regions of the brain, we attempt to meet the first part of the demand chiefly from the focal symptoms which the patient presents, while in the clinical history of the disease, and the general constitution of the sufferer, we seek to find some indication of the nature and cause of the morbid process itself. A very limited acquaintance, however, with the symptomatology of organic cerebral disease is sufficient to prove that in many cases we have to base our diagnosis altogether on the general as distinguished from the focal symptoms. The former class of symptoms accompanying, as they do, to a greater or less extent, pathological processes situated in parts of the brain remote from each other, enable us to do little more than conjecture what the site of the lesion is; we are, consequently, often, in the absence of focal symptoms and of any indication as to the nature of the mischief, unable to form a more satisfactory diagnosis than the vague one of "intracranial disease." Such cases can

be cleared up in no other way than by a *post-mortem* examination; and it is frequently at the autopsy a matter of astonishment that the extensive lesions found after death did not give rise to more distinct focal manifestations during the life of the patient.

The specimens which form the subject of this paper were removed from a patient who was admitted to Sir P. Dun's Hospital last November in an enfeebled state of mind and body. The condition of his intellectual capacity may be gauged from the fact that, though a gardener by occupation, and hence, presumably, with more highly cultivated faculties than the ordinary labourer, he was unable to perform the most elementary arithmetical calculations, to count backwards from twenty, and the only reply to the usual question respecting his age was, "I might be 45 or 60," given in a tone which plainly indicated that he did not think there was much difference between them. His inability for physical exertion was manifested on trying to stand, as, if unsupported, he at once fell, and was quite unable to make any attempt at walking without two assistants. The meagreness of the following clinical notes, taken by Mr. Morton, clinical clerk, is to be largely attributed to the patient's failing memory:—

CASE.—A. B., aged forty-five (?), a gardener, was admitted to hospital under Dr. Finny's care, on the 11th of November, 1891, complaining of severe headache which he states is continuous, of a purulent discharge from his right ear, and of loss of power over his lower limbs. He says that he cannot walk or even stand unsupported, but that if assisted for some time he can do so. No accurate account of the time or order in which these symptoms manifested themselves can be obtained from him, and the general reply to an inquiry directed to the duration of any one of them is that he has had it for three months. An examination of the cranial nerves failed to disclose any lesion which could with certainty be assigned to them. The movements of the eyes were quite free in all directions. There was no facial paralysis, and there did

not appear to be any loss of sensation in the face. Mastication seemed normal, deglutition unimpaired, articulation distinct, movements of tongue good. He stated that his sight was failing, but an examination of the fundus on different occasions showed the absence of any pathological change. His hearing was defective, but this was attributed to a chronic purulent otitis media, with almost complete destruction of the membrana tympani of the right ear.

Upper Extremities.—The grasping power of each hand was quite good. Movements of left arm were not involved, but though he could readily flex his right arm, he had some difficulty in raising his hand to his head, and complained of pain in region of right shoulder on trying to do so. An attempt to ascertain the condition of sensation was unsatisfactory, owing to his contradictory statements.

Lower Extremities.—Power in the lower limbs was diminished, but he was able to draw them up when told to do so, or to raise them off the bed. The deficiency was more marked in the left than in the right. Ankle clonus could be obtained in either limb, but much more easily in the left, in which also the patellar reflex was somewhat more exaggerated than in the right. It was impossible in the lower limbs, just as in the upper, to draw any conclusions concerning his sensation. There was an effusion into his left knee-joint, which subsided considerably.

Bladder and Rectum.—There was no loss of control over these viscera. The urine had a specific gravity of 1022, gave a large precipitate of lithates, but contained no albumen. Constipation was troublesome, as the bowels generally acted only after enemata.

Nothing abnormal was detected by examination of the thoracic and abdominal organs.

On admission his pulse was 72, and respiration 28 per minute. The former, during the course of his illness, on only two occasions rose above 100; and the latter, except once, when it reached 42, varied between 20 and 30. The temperature was very irregular, ranging from 97° F. to 100° F., but not exceeding the latter number except on the evening of the day of his death, when it rose to 101° F.

On the 18th of November vomiting set in, and the emaciation became daily more evident.

The clinical note for 23rd of November is that the patient seems to be decidedly worse. He complains greatly of pain in his head; appears to be getting deafer and more stupid. Pulse (68) and

respiration (42) show a great want of proportion. Vomiting more frequent than formerly. On one or two occasions he complained of pain over the right mastoid process, but there was no superficial œdema, redness, or even increased pain on pressure, when contrasted with the opposite side, to indicate any inflammatory process in the mastoid cells. As a rule, he slept well at night, though he woke up rather frequently, complaining that the pain in his head was so severe that he could not sleep. The stupor and emaciation became more marked day by day, till his death on the 26th of November, just fifteen days after his admission to hospital.

Post Mortem.—The head alone could be examined. On removing the calvaria, and slitting up the longitudinal sinus nothing abnormal was noticed. The dura mater was then incised and reflected so as to expose the pia and arachnoid, but there was an absence of any inflammatory mischief. The brain was then removed, and laid aside for a more detailed examination. The osseous roof of each middle ear was examined, but there was nothing on their cranial aspect suggesting any necrotic process. On removing these, both middle ears were laid open, and were found to contain a quantity of thick, greenish-yellow pus, which on the right side had made its way into the mastoid cells. The ossicles in each ear were somewhat rough, and appeared to be slightly necrosed. There was no indication of any septic phlebitis discovered. *Brain.*—On opening the brain in the ordinary method, the first incision through the left cerebral hemisphere disclosed the presence of two abscesses—one situated about the junction of the anterior and middle thirds, and the other in the posterior third. In the right cerebral hemisphere two abscesses occupying very similar situations were also found. The ventricles and basal ganglia seemed normal. The cerebellar hemispheres contained three abscesses, but the middle lobe was not diseased. A section through the pons showed an abscess situated in the tegmentum. It could, therefore, exercise any injurious effects on the pyramidal tracts only by pressure, and possibly we have in it an explanation of the paresis of the lower limbs. It is, from the site of this abscess, difficult to understand how the motor nuclei of the fifth and sixth cranial nerves and the connection of the auditory with the superior olivary body could have escaped, but no indication of any implication of them was found during life, except possibly in the case of the latter, which may be to some degree responsible for his deafness. The abscesses, though varying in size from a

very small pea to a hazel nut, had many features in common. They each contained similar greenish-yellow, thick pus, had soft and somewhat irregular edges, were encircled with a bright-red inflammatory ring, and showed no well-defined capsule. Sections presented, on microscopical examination, a cavity with an irregular, jagged border, which, owing to its infiltration with leucocytes, stained deeply. Some of the pus was stained and examined for micro-organisms, but with negative results. The much more delicate method, however, of ascertaining the presence of bacteria by inoculating a sterilised tube containing some nutrient medium met with more success. A minute quantity of pus was removed from one of the abscess cavities by means of a sterilised platinum needle, which was then drawn along the slanting surface of solidified agar, and after waiting for some weeks, keeping the tube at the ordinary room temperature, a yellow growth took place along the line of inoculation. This proved to be a micrococcus, but its exact nature has not yet been determined.

The methods of the formation of multiple abscesses of the brain are very interesting. Abscesses arising from local causes, of which injury and disease of the ear are the most potent factors, are almost invariably single. Of 27 cases of abscess of the brain attributable to ear disease, collected by Gull and Sutton, there were only 2 in which more than one abscess was present; in 1 of these there were three abscesses in the cerebellum, and in the other, one in the cerebrum and one in the cerebellum.^a Morbid processes in distant parts of the body from which septic matter finds its way into the circulation are responsible for a large proportion of the cases in which the abscesses are multiple. Of 14 cases of general pyæmia in which the brain was involved, in 8 the foci of suppuration were numerous, and were scattered through different parts of the cerebrum. Secondary suppuration in general pyæmia would appear to be seated much less frequently in the brain than in some other organs, for out of 234 cases of cerebral abscess

^a Reynolds. System of Medicine. Vol. ii., art. "Abscess of the Brain."

collected by Gowers^a there were only 9 cases of pyæmic origin. There is, finally, a curious group of cases of cerebral abscess secondary to suppuration elsewhere, but which present none of the symptoms of pyæmia, and after death a most careful examination fails to disclose any metastatic abscesses except those in the brain. The limitation of the secondary suppuration to the brain is very remarkable and at present inexplicable. "These cases," writes Gowers, "are more numerous than those in which there is general pyæmia, and about ten per cent. of all cases of cerebral abscess are of this origin (25 of 234 cases).^b Fagge states that within a few years as many as 6 cases of this kind occurred at Guy's Hospital.^c Gull was one of the first observers who directed attention to the fact that, in the majority of these cases, the primary suppuration is connected with the lung. Of 11 cases collected by him in which purulent deposits took place in the brain without the intervention of general pyæmia, in 7 the abscesses were multiple, the original focus having been connected with tubercular phthisis, acute pneumonia, simple bronchitis, or bronchiectasis.

In the case under consideration at present, owing to the impossibility of obtaining a complete *post-mortem* examination, it is difficult to say with certainty of which of the two varieties it is an example. From the number of the abscesses (at least eight were found), from their situation, scattered as they were through different parts of the brain not having any direct anatomical relationship with the ear disease or with each other, from the similarity in character which they presented, there can be little doubt that they were secondary to suppuration elsewhere, and that the

^a Gowers. *Diseases of the Nervous System*. Vol. ii., p. 442.

^b Gowers. *Loc. cit.*

^c Fagge. *Practice of Medicine*.

infection was conveyed by either the blood-vessels or lymphatics, most probably by the former. The only evidence in favour of general pyæmia is the effusion into the left knee, and the pain in the right shoulder. These are, however, explicable on other theories, and against the general pyæmic hypothesis we have the absence of cardiac phenomena, of rigors, profuse perspirations and very high temperatures, of an enlarged and tender spleen, of jaundice and albuminuria. The weight of evidence would appear, therefore, to point to this case as an example of the formation of multiple abscesses of the brain from a primary source of suppuration without the intervention of general pyæmia. The presence of the double purulent otorrhœa, and the absence of indication of pus of old standing in any other part of his body render it probable that the former was the source of infection.

In conclusion, I wish to express my sincere thanks to Professor Finny, President of the Royal College of Physicians of Ireland, under whose care the patient was admitted to hospital, for permission to report this case, and to Messrs. Kennan, Montague Griffin, and Wynne, for the illustrations of the specimens which they kindly made.

CASE OF MEDIASTINAL LYMPHO-SARCOMA.

BY J. W. MOORE, M.D. UNIV. DUBL.; F.R.C.P.;

Physician to the Meath Hospital.

[Read in the Section of Pathology, January 22, 1892.]

THE differential diagnosis between an intra-thoracic tumour and a pulmonary consolidation or a pleural effusion must always be a matter of deepest interest to the physician, and of supreme moment to the patient. In the following case, certain elements presented themselves which rendered an accurate diagnosis more than ordinarily difficult. The case also illustrates the importance of making an examination after death in every case of fatal disease.

The patient had the advantage of advice from Mr. Daniel P. Coady, L.R.C.P. Edin., of Johnstown, county Kildare, before she came under my care at the Meath Hospital. It will be seen from the following extract of a letter from Mr. Coady that the nature of the case was more than suspected by him. He wrote to me:—“I could never make it out a case of phthisis, although it was often diagnosed as such. At last I began to think that my ears should be going wrong, so it was a great satisfaction to me to hear the result of the *post mortem*. I always suspected there should be some intra-thoracic tumour. The dulness was only in front; there was fulness in place of the usual sinking under the clavicle; the cutaneous veins were greatly distended; and there were some enlarged lymphatic glands in the subclavian and occipital triangles. The latter disappeared under iodide of potassium and iodide of iron.”

CASE—Kate K., aged twenty-five, single, from the county Kildare, was admitted to Ward 18 of the Meath Hospital, under my care, on Thursday, November 26, 1891. She had been in delicate health for the previous eleven or twelve months, a troublesome dry cough and shortness of breath being the chief symptoms since August, 1891. By her friends she was supposed to be in "consumption"—more particularly as she had been losing flesh for six months or so.

When I first saw her, on the morning of November 27, she was in great distress with her breathing, and sat bolt upright in bed. She was by no means extremely wasted, and the congested look about her face and eyes at once suggested that she was perhaps the subject of uncompensated valvular disease of the heart. Physical examination, however, failed to elicit any evidence of cardiac disease. Her pulse was 102; respirations, 38; temperature, 99.8°. The external jugular veins were permanently full.

Examination of the chest soon showed that an extensive pleural effusion existed on the right side. Thus, this side was fuller than the left, the ribs at the base were rather widely separated, there was relative inaction of the side. On percussion, very complete dulness was detected all over the front of the right chest, except in the acromial angle, where the sound was comparatively clear but high-pitched. The dulness transgressed the middle line above to the extent of one and a half to two inches. Posteriorly absolute dulness ruled all over the base, but in the inter-scapular and supra-scapular regions the percussion note was fairly clear, and breath sounds were audible. So far as I had gone in the examination, I believed we had to deal with an extensive right pleural effusion, but one thing puzzled me, and that was the slight displacement only to the left which the heart seemed to have undergone. The apex beat was in the nipple line.

Notwithstanding this fact, I considered that the evidence of pleural effusion was convincing, and the patient's great distress led me to advise thoracentesis. Accordingly, the same afternoon, Dr. Lennon aspirated and drew off 10 ounces of ordinary sero-fibrinous fluid. Next morning the girl was able to lie down, which she had not done for many weeks, and expressed herself much relieved. On proceeding to examine the chest, I was greatly surprised to find that there was practically no change in the area of dulness over the right apex in front, and that the jugular veins on both sides of the neck remained quite as full as they had been before thoracentesis.

For a time the patient felt better, but distress and orthopnoea gradually increased, so that on December 19, 1891, thoracentesis was again performed. Only two or three ounces of sero-fibrinous fluid were drawn off, the cannula apparently becoming plugged with a dense mass of coagulated fibrin. The fluid had a specific gravity of 1023, and was highly albuminous. The same evening her temperature ran up to 102.1° —the highest reached during the illness. There was again no change in the physical signs over the right apex. The infraclavicular region now began to bulge manifestly, the superficial veins were large, and the skin looked puffy or œdematous, and pitted slightly on pressure. I thought that there must be either an encysted pleural effusion (probably an empyema) or an intra-thoracic tumour pressing on the innominate veins.

On December 26 the patient took a change for the worse, her temperature became subnormal, and she gradually sank and died on Tuesday, January 5, 1892. I saw her for the last time on New Year's Day. In my unavoidable absence Dr. Lennon kindly took charge of her, and after her death Dr. Craig made a careful, though partial, autopsy. To Dr. Craig I am indebted for the following note of the *post-mortem* appearances:—

Autopsy.—The thoracic cavity only was examined. On removing the skin over the sternum and costal cartilages, the subcutaneous tissue was found to be adherent and occupied by a new growth which had pushed its way through the pectoralis major and the intercostal muscle between the cartilages. On attempting to elevate the sternum, it was necessary to cut through a tumour which lay in the anterior mediastinum, and which was adherent to the bone. On the right side the pleural cavity was filled with fluid, the upper and lower lobes of the lung, but especially the latter, being compressed backwards and inwards against the spinal column so that no lung tissue was apparent. The fluid was straw-coloured and filled the pleura to the apex, while stretching across the cavity at intervals from above downwards were net-like partitions of elastic lymph which came away on the hand like cobwebs. The pleura was not thickened. On turning to the tumour which occupied the anterior mediastinum, it was found to be adherent to both lungs and growing far into the right one; it was also adherent to the pericardium, the inner surface of which was lined by the new growth. Nodules also lay between and were adherent to the pleura and pericardium. The growth extended up into the neck as far as the lower border of the larynx.

The pneumogastric nerve passed through the mass of the tumour, but was not flattened through pressure. The neoplasm, as is usual in such cases, grew round rather than into the nerve. Closely surrounding the trachea were many enormously enlarged glands, particularly on the right side. The œsophagus on the left was not in any way involved or compressed.

Dr. H. C. Earl, F.R.C.P., very kindly made a careful histological examination of the tumour, which was a typical hard lympho-sarcoma. He also favoured me with a beautiful stained section of the growth, which shows under the microscope numerous round cells and the stroma as well.

The new growth probably took its origin in the remains of the thymus gland.

Remarks.—It is to be regretted that circumstances forbade a more complete *post-mortem* examination. During life, however, there were no evidences of splenic or other visceral disease—certainly there was no pericarditis. The way in which the tumour grew round the trachea and pneumogastric nerve is noteworthy. In his recent work on the “Pathology of Mediastinal Tumours,”^a Dr. J. Lindsay Steven, of Glasgow, observes that perhaps the most characteristic feature of mediastinal lympho-sarcomata is the manner in which they mould themselves round the great tubular and vascular structures of the upper portion of the thoracic cavity. It is, indeed, true that the veins suffer from the encroachment of the tumour much more severely than the arteries. Dr. Steven also points out that sarcomata of the mediastinum seem to behave differently to aneurysm as regards their mode of involving the nerves. Aneurysm pushes the nerve trunk aside, crushes and destroys it by pressure. Sarcoma, on the other hand, appears to surround it much in the same way as it does a bronchus or an artery.

In my case, lastly, the singular freedom from inflammation shown by the involved pericardium is very noteworthy.

^a The Pathology of Mediastinal Tumours, with special reference to Diagnosis. By John Lindsay Steven, M.D., Assistant Physician and Pathologist, Glasgow Royal Infirmary. London: H. K. Lewis. 1892. 8vo. Pp. 100.

ADENOMA OF THE BREAST IN CHILDHOOD.

By R. GLASGOW PATTESON, M.B. UNIV. DUBL.; F.R.C.S.;

Assistant Surgeon to St. Vincent's Hospital, Dublin.

[Read in the Section of Pathology, Feb. 26, 1892.]

THE occurrence of adenoma—or “glandular tumour”—of the breast below the age of puberty is one of such rarity, if, indeed, it is not unique in pathological record, that no apology is needed for describing in some detail two cases which have recently come under my observation.

The first occurred in the practice of my colleague, Mr. M'Ardle, and to his kindness I am indebted for the opportunity of examining it and determining its histological character. The girl, who was just thirteen years of age, was sent up to St. Vincent's Hospital from the south of Ireland last October, supposed to be suffering from chronic abscess in the left breast. In the absence of Mr. M'Ardle she first came under my notice, and it was at once evident that we had to deal with a solid growth and not with an abscess. There was a history of a fall and injury to the breast about a year previously, and to this the growth was supposed to be due; at any rate, it was first noticed one or two months after the accident. It had gone on slowly increasing in size, absolutely without pain, tenderness, or any other symptom. The child's general health was excellent, and she looked well and was well nourished. There was no history of a tubercular tendency obtainable. There was commencing development of the normal gland, which was freely movable over the growth. The tumour was about the size of an almond, irregularly oval in shape, and indistinctly lobulated in outline; it was freely movable over the underlying tissues, quite

Fig. 1.

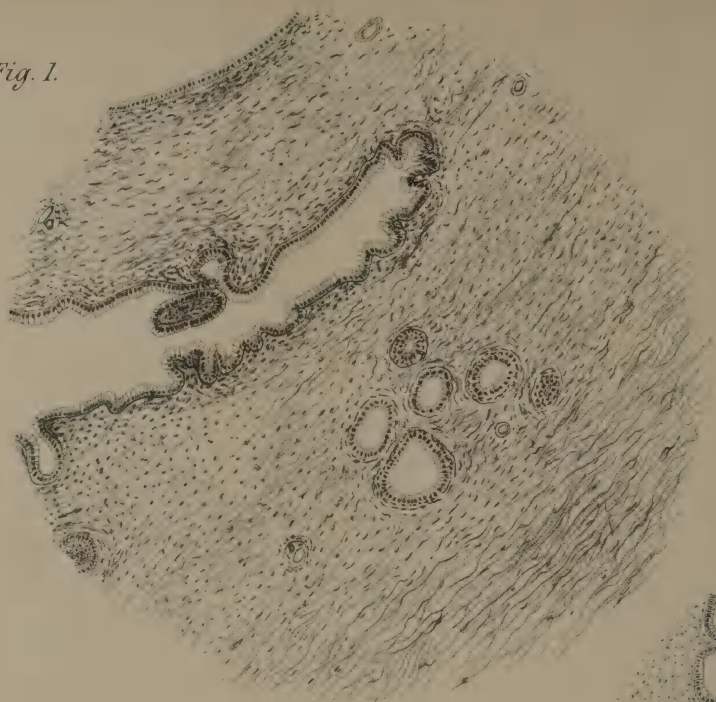
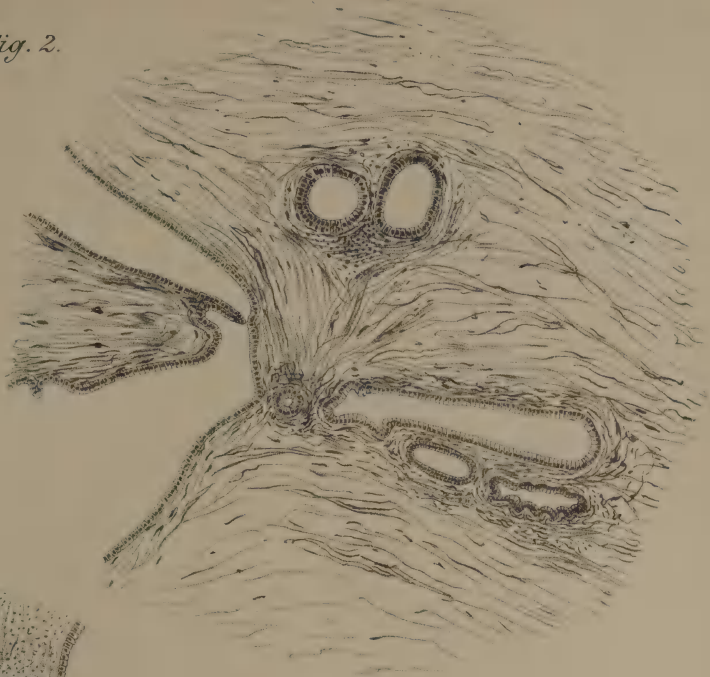


Fig. 3.



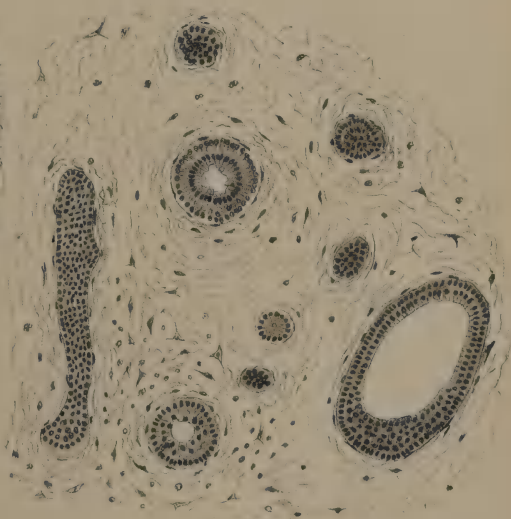
Fig. 2.



5.



Fig. 4.



painless to pressure and gave a sense of elasticity, though not of fluctuation, to the touch. Considerable doubt was expressed as to its nature. The age of the patient precluded, it was thought, an adenomatous growth; and it was regarded as most probably of a sarcomatous or of a chronic inflammatory nature. It was removed by Mr. M^rArkle, who kindly put it at my disposal for examination. As it was immediately placed in strong alcohol, no opportunity was afforded me of observing its naked-eye characters.

In the hardened condition it measured 28 mm. in length, 17 mm. in width, and 9 mm. in depth—all outside measurements. It cut somewhat toughly with a firm and uniformly white surface. On examining sections under the microscope, it was found that the greater portion of the growth consisted of a dense fibrous tissue arranged in wavy parallel bundles, and for the main part but sparsely cellular in type. Scattered through this fibrous stroma were numerous groups of glandular acini, some small and round, others large and irregular in shape, which for the most part showed a distinct lumen, and were everywhere sharply defined from the surrounding fibrous stroma by a well-marked basement membrane. They were lined with a simple, or in places compound, columnar or cubical epithelium. Many of the acini had undergone a certain amount of distension, or of “cystic degeneration,” and in these many papillary ingrowths projected into the lumen of the tube, giving rise to a sinuous and irregular outline. Many, again, had the lumen obliterated by the actively proliferating and degenerating epithelium, which had here lost its columnar character, except in those cells immediately adjoining the basement membrane. The varying appearances are well seen in the section from which Fig. 1 [Pl.] was drawn. It will be observed in this, that in the immediate neighbourhood of the acini the fibrous tissue is more cellular and less wavy in type.

The second case was that of a child aged twelve years and nine months, who was brought to St. Vincent's out-patient department towards the end of January. She was suffering from a lump in the right breast, and gave the following history:—Three months previously the mother noticed a small “kernel,” which was painless, and after a little time apparently went away. About a month later it was again noticed, this time giving rise to pain, and it was also found to be gradually getting larger, [until, at the end of nearly two months, it had attained the size, and very much the shape, of a small fig. There was no trace of mammary development. The growth was seated right below the nipple, which apparently was freely movable over it, as likewise was the growth over the underlying tissues. It was slightly irregular in outline, giving the sensation of lobulation, and was very tender and sensitive on handling—even the rubbing of the clothes causing pain. It was not continuously painful, but the pain came in darts, and was said to be worse at night, though this statement was afterwards varied. Otherwise the child's health was good, and except for this painfulness, which exactly resembled the so-called “neuralgia of the breast,” so often associated with these tumours in young and nervous women, she suffered no inconvenience. However, on account of this, and as the growth was increasing, I recommended its removal, regarding it from the experience gained in the previous case, and the exact identity of the clinical features (except the pain), as another example of adenomatous growth. Accordingly, on the 28th January, I removed the tumour by a small semilunar incision about half an inch below the margin of the areola. The tumour was definitely encapsuled, and was freely detachable underneath, but in front and above the capsule was adherent to the skin, so that at the upper limit, about an inch above the nipple, a small elliptical piece of skin had to be removed in order to ensure

the complete removal of the growth. The wound united throughout by first intention, and the child was going about at the end of a week.

On making a section of the fresh tumour, which measured 35 mm. in length, 27 mm. in breadth, and 13 mm. in depth—all extreme measurements—the cut surface presented a peculiar, pearly-white, in parts almost translucent, appearance, as if composed of a collection of half-boiled grains of sago—in each a dull, opaque, whitish centre, with a clear, greyish, translucent halo round it. It was quite soft on cutting into it, and exuded a clear mucinoid juice. On examining sections under the microscope, it was found to resemble very closely in its general features the growth described above. The fibrous inter-acinous stroma was, however, finer and more delicate in texture, and was composed of slender interlacing bundles of connective tissue, between which lay long flattened nuclei. These were few in number, except in the neighbourhood of the acini, where in many parts a distinct small-celled infiltration had occurred. The general features are well shown in Fig. 2. Under a higher power it was seen that in many of the larger acini active cell-proliferation was going on, taking in many cases the form of epithelial ingrowths. These, it could be seen, consisted at first merely of a heaping up and protrusion into the lumen of the growing lining epithelium, which at the same time lost, except near the basement membrane, its columnar type. In a more advanced stage it was seen that a fine core of delicate connective tissue was now projected into the mass of proliferating cells, thus forming a distinct papillary ingrowth, so that the lumen of some of these distended acini was almost obliterated by the intra-acinous growths. These stages of formation are illustrated in Fig. 3. The drawing also shows in the neighbourhood of the acini the more cellular type of the stroma and the small-celled infiltration which

surrounded many of them. These cells were embryonic in type, and were either proliferating tissue cells or an exudation from the blood-vessels. I incline to the former view, although in some places they seemed to bear a definite relation to the vessels, as in Fig. 3; but in no place were they sufficiently numerous to justify the view that any sarcomatous development was in process. Further, in no place could the evident epithelial proliferation be found to transgress the limits of the basement membrane, it was always intra-acinous, so that in no part did the growth show the characters of an infiltrating, or "destructive" (*destruirendes*) adenoma.

Based on their microscopic characters, therefore, from the relative amount of fibrous stroma and glandular tissue, these tumours must be classed as fibro-adenomata. They differ from the *true* or simple adenoma met with in the breast and represented in Fig. 4, by the relatively greater proportion of fibrous to glandular tissue, and also by the character of the inter-acinous stroma. This in the true adenoma is an excessively delicate tissue, composed of a network of branching, stellate cells, and of cells with oval, vesicular nuclei lying between these fine fibres. In this pure form, adenoma is one of the rarest of breast tumours, the majority of cases conforming to a fibro-adenomatous type.^a The two growths which are recorded above are interesting again as presenting an intermediate stage between the simple adenoma and the proliferating cystic adenoma which occurs in the breast, but is more commonly met with in the ovary. I have given a drawing of this condition for comparison in Fig. 5. It was removed from the breast of a woman aged fifty. Here, with marked dilatation of the acini and secondary disappearance of the intervening stroma, we have well-marked papillary

^a The tumour represented in Fig. 5 was removed from the breast of a young unmarried girl. It is the only example of pure adenoma of the breast I have met with.

intra-cystic growths, such as are so frequent in the cyst-adenoma papilliferum of the ovary. But the sequence shown in these drawings proves most conclusively the identity of the process, and is most instructive in the light it throws on the successive stages. As long as the epithelial proliferation remains *intra-cystic*, we have the comparatively benign proliferating cystic adenoma; but, if the epithelial development becomes *extra-cystic*, and crossing the basement membrane runs riot in the inter-acinous stroma, then we have, on the other hand, the infiltrating adenoma, or early stage of the adeno-carcinoma, with its rapid local malignancy and distant metastatic deposits.

The rarity of such adenomatous tumours even in early adult life is well recognised. Gross analyses 23 examples of these growths—Only 4 occurred between the tenth and twentieth year; 6 between the twentieth and thirtieth, a proportion which holds almost constant up to the fiftieth year, after which age only two cases are included. “Of the entire number not a single one occurred before the sixteenth year, or during the developmental state of the mamma; 16, or 69·5 per cent., appeared previous to the fortieth year, or during the period of the greatest functional activity of the breast.”^a Of 55 cases observed by Velpeau, only 8 occurred between the ages of 15 and 20, and not a single one before the fifteenth year; and of 130 other cases collected by him, 35 occurred between the ages of fifteen and thirty; but, again, none earlier than the fifteenth year.^b “These ‘*adeno-fibromata*,’” writes Mr. Bryant, “are found, as a rule, in the young and unmarried, and in the apparently healthy and robust. . . . In women the majority of these cases begin to grow, or are

^a Gross. “Tumours of the Breast.” Amer. Syst. of Gynecol. and Obstet. Vol. II., p. 266. Edinburgh. 1888.

^b Art. “Mamelles.” Nouv. Dict. de Méd. et de Chir. Prat. Tome XXI. Paris. 1875.

first discovered, between the ages of twenty-one and thirty, although, as shown by my table, almost as many begin in younger people, at or after puberty; at later periods of life they less frequently originate, although they may be frequently found to exist in them.”^a Mr. Bryant gives the results of “100 cases consecutively observed, seen, and analysed,” and of these “27 cases were first observed between puberty and the age of twenty—that is, during the development stage of the breast’s life;” but not one of these cases occurred under puberty, and no mention is made of any such case being on record. Indeed, it would appear from the literature of the subject that *a growth of any kind in the breast before its period of development or of functional activity has been hitherto undescribed.* Sir James Paget writing of “mammary glandular tumours” and their rate of growth, says:—“One was removed from a woman twenty-seven years old; it was observed for fourteen years, and in all that time it scarcely enlarged; yet after this it grew so rapidly that in six months it was thought imprudent to delay the removal.”^b This, if the dates are reliable, would place its development from the the fourteenth year, and is the earliest example I can find recorded.

One point of peculiar interest in my second case is the entire absence of any trace of glandular development on the healthy side. But that an adenoma should occur before the secreting tissue of the breast has shown signs of development is, however, no more remarkable than that such a growth should form after the gland has ceased to be functionally active, and has under natural conditions undergone fibrous atrophy. Yet Velpeau records a case occurring in a woman aged 85! The occurrence of these tumours at the extremes of life must, therefore, I think, be explained by a common

^a Diseases of the Breast. P. 102. London. 1887.

^b Lectures on Surg. Pathology. 4th Edition. P. 560. London. 1876.

theory ; and to such a theory a leading pathologist has already given expression. Writing of the occurrence of adenomata in general, and the absence of any special age liability—in so far as such growths have been observed to occur congenitally or in the earliest years of life as well as in extreme old age—Birch-Hirschfeld remarks :—“ It is, however, probable that those glandular tumours which first become apparent in later life are in reality the outcome of embryonic rudiments.”^a Whatever light the future may throw on the origin of heteroblastic growths at all periods of life, we must be content for the present to let the question lie enveloped in the same darkness that surrounds many other problems of the pathology of to-day, if we decline to accept the only alternative—the theory of “embryonic rudiments.”^a

^a Art. “Adenom.” Eulenburg’s Real Encyclopädie der gesam. Heilkunde. Vol. I.

ENDOTHELIOMA OF THE LESSER OMENTUM.

By CONOLLY NORMAN, F.R.C.P.;

Medical Superintendent, Richmond (Dublin District) Asylum.

[Read in the Pathological Section, February 26, 1892.]

THE following case is of some interest both from a clinical point of view and on account of the rarity of tumours of the position and structure here shown:—

CASE.—S. M., aged seventy-five, was admitted to the Richmond (Dublin District) Asylum, May 8, 1891. He was a tall, well-nourished man, heavily built, and very fat.

Nothing satisfactory could be made out with regard to his hereditary history.

He had been insane off and on for a great number of years, and had been under treatment in the Richmond Asylum for three months in 1852, for four months in 1854, for six months in 1856–57, and again for three months in 1860. Unfortunately no clinical records exist for those periods, though probably this is unimportant for our present purpose. Shortly after his discharge in 1860 he was committed to the Criminal Asylum, Dundrum, and remained there till he was transferred to the Richmond Asylum in May, 1891.

For his earlier mental symptoms one had to depend chiefly on his own account of himself. He used to entertain beliefs as to a mission or authority committed to him to overthrow the constitution of the country, and under their influence tried to damage public buildings and injure property marked with the crown. He stated that he had been sent to the Criminal Asylum after having been arrested for smashing the windows of a Government office.

When he came under my care he was a garrulous, weak-minded old man, little troubled by the shadows of his former delusions, not hypochondriacal nor affected with visceral delusions.

The existence of a tumour in the abdomen escaped attention on his admission, perhaps owing to the fact that he was exceedingly fat. Early in June, 1891, he suffered from an attack of pain in the right hypochondrium, with constipation, vomiting, and raised temperature. It was then found that the hepatic dulness extended as low as the level of the umbilicus on the right side. Save in the epigastric region, dulness could scarcely be detected left of the

middle line. The dull area was slightly prominent. The lower edge could be distinctly made out, corresponding to the margin of a mass which moved with the movements of the diaphragm, and which could not be distinguished by palpation from the liver. This mass produced the impression of being globular, presenting everywhere a rounded outline; it was very hard. The abdominal parietes did not at this time present any enlarged veins. The diagnosis was formed of cancerous infiltration of the liver chiefly affecting the right lobe. The extreme hardness of the tumour seemed to exclude the possibility of its being an hydatid, though the rounded outline and the absence of cachexia pointed in that direction.

The patient stated that he had been liable to similar attacks for the last twenty years, and he sorrowfully attributed them to hard drinking in early youth. He was ignorant of the existence of a tumour.

The symptoms passed off, but returned again at the end of the month, and on June 30 there was hæmatemesis and melæna.

The tumour slowly and steadily increased in size. It preserved its generally rounded contour. There was, on the whole, little pain. What there was varied in character, being sometimes sharp and colicky, at others dull and gnawing; but until the last six weeks of life he was often quite without pain for several days at a time. The temperature was usually about 99° and ran up to 100° when attacks of pain came on. Emaciation gradual, not extreme. From time to time attacks of hæmatemesis and melæna, usually contemporaneous; sometimes slight hæmatemesis without melæna or *vice versâ*. The bowels were generally free, and constipation was never a marked symptom. About one week before death he became slightly jaundiced; till then there had been no sign of jaundice. Neither was there any dropsy till a fortnight before death, when the feet and legs became slightly œdematous.

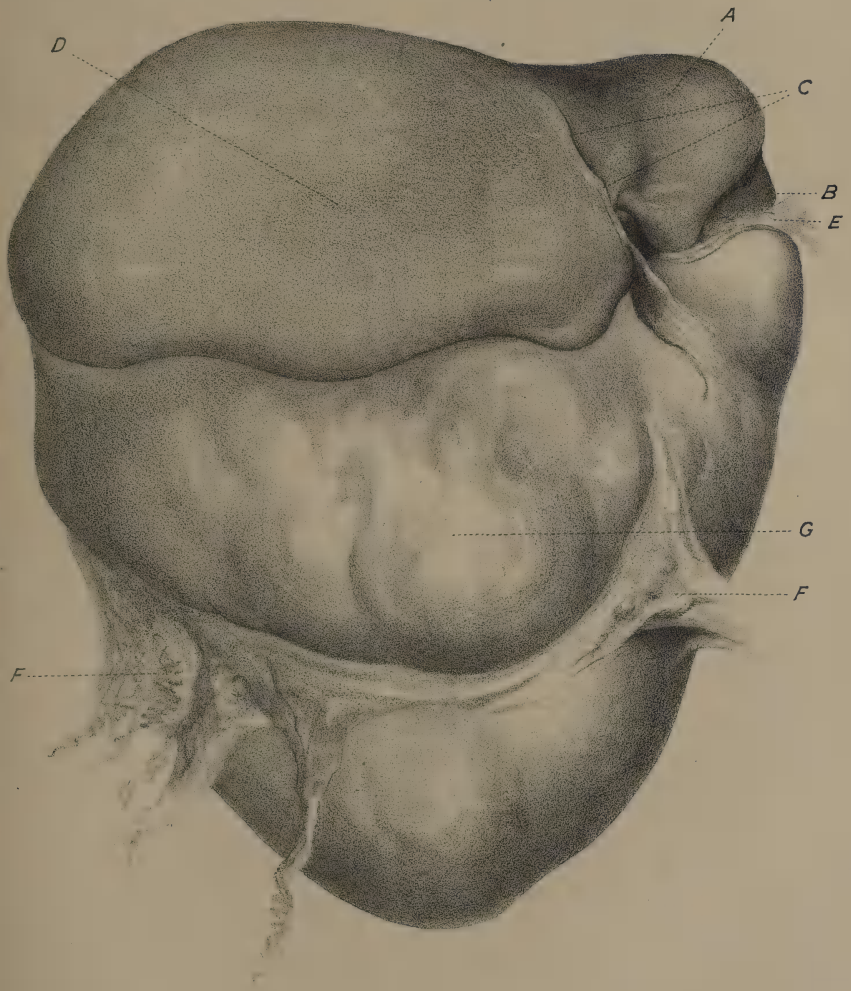
The tumour grew by extension downwards and towards the left. It could never be demarcated by a groove or depression, nor by the intervention of a clear percussion note, from the liver. It preserved to the touch a generally rounded surface and outline, but as the patient grew thinner slight unevennesses were to be detected here and there. While it remained very firm throughout, the sense of resistance to deep pressure became less towards the end, suggesting central softening.

The patient died 6th February, 1892. Autopsy 14 hours after death. Body slightly jaundiced; somewhat wasted, yet there was a thick layer of superficial abdominal fat. The belly was distended

by a large tumour filling the entire right side of the abdomen down to the level of the mid hypogastric region, and in the umbilical region extending about three inches to the left of the navel.

The brain exhibited the usual indications of chronic wasting with thickened pia-arachnoid and slightly atheromatous arteries of the base. Lungs and heart healthy. Aorta somewhat atheromatous. Kidneys normal. Spleen normal. There was no indication of general peritonitis, and no effusion into the peritoneal cavity. A tumour was found occupying the position above indicated and displacing the intestines. It proved to be not the liver, but a growth lying inferiorly to that organ, of a shape which may be described as a broad-based, blunt-pointed cone. The liver was small, and of a uniform pale colour. It was very much pushed upwards, the most convex portion extending on the right to the level of the fourth rib. The liver was twisted on itself in such a way that very little of the left lobe appeared anteriorly. The inferior margin was very thin, and lay overlapping the tumour, which bulged below it, so as to leave no groove between the organ and the new growth. The left lobe of the liver was not adherent to the tumour in front. The right half of the overlapping portion of the right lobe adhered uniformly to the tumour, but the adhesions were very easily torn; the left half was attached here and there by a delicate band of adhesion. The surface of the liver retained its normal glisten; the overlapping edge was somewhat rough, as if cirrhotic; elsewhere the surface was smooth. The gall-bladder did not appear beneath the edge of the liver, whereby this case differed from that of Frerichs to be presently described. On raising the overlapping portion of the liver in the position of the notch for the gall-bladder a smooth sac-wall could be distinguished embedded in the tumour at its most convex superior portion. This I took to be the anterior wall of the gall-bladder. Slitting it open longitudinally one found a cavity, the posterior wall of which was formed by softened tumour, here almost diffuent. Passing a probe upwards, to endeavour to trace the cystic duct, one came on a large, round gall-stone; no duct could be discovered.

The anterior surface of the tumour was adherent to the anterior abdominal wall by a few delicate bands. The colon from about the middle of the ascending to the middle of the transverse portion adhered to the lower and anterior surface of the tumour, but could be everywhere readily stripped off from it without injury. The great omentum appeared to be stretched over and entangled in the lower portion of the tumour; in parts it could, with care, be torn



To illustrate Mr Conolly Norman's Paper.

off. The stomach was not adherent to the tumour, was not distended, and did not exhibit any new growth. It contained a quantity of thick, ropy, blood-stained mucus. From the lesser curvature to the upper surface of the tumour, just beneath the left lobe of the liver, ran a short band, which appeared to be a portion of the lesser omentum remaining unaltered. The duodenum curved sharply behind the tumour, to which it did not directly adhere, but was connected throughout the greater part of its length by a short structure having the appearance of mesentery. A probe introduced into the common bile-duct ran upwards about three-quarters of an inch through a fold of peritoneal tissue, and was then occluded in the substance of the tumour close to its posterior margin. The portal vein was also lost in the new growth and occluded in the same position.

The pancreas was free, and its structure was normal.

When the liver and tumour were removed and the mass examined from behind and beneath, the left lobe of the liver was found to be free, and also the left half of the lobulus quadratus and the greater portion of the lobulus Spigelii. The tumour was inseparably adherent to the liver in the region of the transverse fissure and to the adjacent portions of the right lobe (lobulus quadratus, &c.).

The mass of tumour and liver together weighed 7 lbs. (112 ozs.). Of this I should say, from its size, the liver composed not much over 40 ozs.

The tumour was of a pinkish-grey colour, somewhat uneven and lumpy on the surface, without being distinctly nodular. In the fresh state it was easy to tear off from the surface shreddy patches of thin membranous tissue. On section the growth was found to be surrounded by a fibrous-looking capsule, of thickness varying from a line to little over one-eighth of an inch. From the internal surface of the capsule bands of a similar tissue ran inwards at a very acute angle. Some of these could be traced for a considerable distance into the interior of the tumour, uniting here and there with each other, and again splitting off at an acute angle. These bands were of a lighter colour than the rest of the growth, which was distinctly redder. The external portion of the tumour cut firmly; towards the centre it became more soft, but nowhere except around the back of the gall-bladder was it soft with the softness of encephaloid. Interiorly there were several hæmorrhages, and the centre was occupied by a large clot.

The liver on section was very pale. The bile-ducts were empty and were not dilated. In the middle of the overlapping portion of

the right lobe, surrounded by liver tissue, was an irregular nodule of mottled, whitish discoloration; at its widest part of about the diameter of a sixpence. It was of equal firmness with the liver tissue. No other new growths were found throughout the liver nor in any other viscera, though careful search was made. The mesenteric and retro-peritoneal glands were not enlarged. The intestines throughout showed here and there patches of intense injection, and the ileum contained a little blood.

Microscopic examination of the tumour showed a structure resembling that of an alveolar sarcoma. The fibrous capsule consisted of adult connective tissue with scattered fibres of yellow elastic tissue. The larger bands passing into the interior of the tumour contained similar constituents, and also many elongated connective tissue cells with rod-shaped nuclei. As the bands broke up further, to form the walls of the alveoli, they consisted only of the latter tissue. The larger bands contained vessels with well-formed walls. In the smaller the vessel-walls were formed by connective tissue cells identical with the surrounding tissue. The bands divided again and again into alveoli of various sizes—some simple, some compound—all tending towards a spindle or lozenge shape. The contents of the alveoli were cells containing little protoplasm, with large, granular, and bluntly-oval nuclei, staining deeply in carmine and logwood. In the smaller alveoli, and in the sub-divisions of the compound alveoli, the cells tended to arrange themselves in rows in the direction of the long axis of the alveolus. Towards the margin of the tumour the alveoli were of an elongated lozenge shape, and often contained at one or both extremities but one row of cells. Exceedingly delicate fibrils could be distinguished passing from the walls of the alveoli between the cells, and it appeared as if each cell was surrounded by such fibrils. The cells in contact with the walls of the alveoli exhibited a tendency to adhere to them even in teased specimens. The central portions of the tumour exhibited the same general structure as the outer portion, but the connective tissue was less in quantity, the alveoli were larger, and tended more to be circular in outline, and very densely packed with cells. Owing to these conditions it was difficult to obtain satisfactory sections of the interior parts of the tumour. There were numerous microscopic hæmorrhages in the outer parts; these became more frequent and larger within. Where the liver and tumour were intimately bound together in the neighbourhood of the transverse fissure, sections showing both structures exhibited to the naked eye a fairly-marked line of demarcation.

Microscopically this proved to be non-nucleated connective tissue. Septa ran off at an acute angle into the tumour, enclosing lozenge-shaped groups of cells, as at the margin of the tumour elsewhere. Quite a similar arrangement existed on the liver side of what we may call the capsule of the tumour. The latter structure was in some places very thin, and, as it were, encroached upon by the intercalation of elongated groups of tumour cells on one side and of liver cells on the other, so that these were but separated from each other by a very narrow band of connective tissue; but I found no place in this position where cells similar to those of the tumour occurred on what was distinctly the liver side of the capsule. The liver cells themselves were small and misshapen, no doubt owing to the pressure exerted by the abundant connective tissue.

The parts of the liver most remote from the tumour exhibited a slight degree of cirrhotic change of the "alcoholic" type. This was very well marked in the thin anterior portion that had been stretched over the tumour. The discoloured nodule observed by the naked eye contained clusters of tumour cells. These were situated in the thickened interlobular connective tissue, which here and there spread out to form a well-marked capsule for the oval or circular group of cells. The cells and their arrangement within their connective tissue capsule were identical with those observed in the tumour, the only difference noticeable being that the entire groups of cells tended to be more circular in shape than in the tumour. The clusters of cells were isolated from each other, not only by their proper capsules but generally also by lobules of liver tissue.

Manifestly, from its situation, this growth can only have originated from the gall-bladder or from the lesser omentum. A moment's thought will be sufficient to show that the notion of the former origin is untenable. The structure of the tumour appears to suggest that it is a product of the proliferation of the endothelial cells lining the lymphatics of the omentum. Probably its mode of growth was by the newly-formed cells insinuating themselves between the layers of the connective tissue and continually splitting it up into increasing numbers of loculi. Possibly the direction of increase was at once exogenous and endogenous, the outward growth of the cell masses being accompanied by constant formation of fresh connective tissue, which was again split

up into alveoli; while the inward growth consisted merely or chiefly in the further intrusion of cells between the layers of the connective tissue, hence the lessened consistence of the central portions.

The clinical interest of the case centres in the extreme difficulty of diagnosis in such a condition. I know not how a case of this kind could be diagnosticated by physical signs during life from a malignant growth in the liver. The absence of ascites throughout and the almost complete absence of jaundice were remarkable. Why complete occlusion of the vena portæ and the gall-ducts did not occur sooner it is not easy to understand.

It is interesting to note the slight tendency which seems to have existed to secondary deposit—one small depot in the liver being alone discoverable.

Tumours of the lesser omentum must, I apprehend, be extremely rare. My opportunities for investigating the literature of the subject are not perhaps very complete, but I have only been able to find one case recorded which pretty closely resembles mine. It is briefly described and figured by Frerichs, "*Diseases of the Liver*," pp. 309, 310, vol. II. (New Sydenham Soc. Trans.). There are, says this author, cases "where cancerous tumours of the lesser omentum correspond closely to the form of the liver, and where it is impossible to distinguish between them and cancer of the liver" during life. Frerichs' illustration shows a small and compressed liver, stretched, as it were, over the superior surface of a tumour about twice its size, the latter being more distinctly nodular than in my case, and presenting what appear to be somewhat similar bands of peritoneal attachment. In this case the gall-bladder protruded beneath the liver and above the tumour, being much enlarged from (? early) occlusion of the cystic duct.

Primary malignant growths of the omental structures are most frequently found in the great omentum, but, even

there, are of very unusual occurrence. Samuel West, describing a case of primary disseminated cancer of the omentum, calls attention to the rarity of primary cancer in this situation, and says that only two cases—described respectively by Gibbon and Longmore—are to be found in the Transactions of the Pathological Society (London). Here, however, he is wrong. He has overlooked two cases by Bristowe and one by Crisp.

Bristowe's first case is of some importance for our present purpose, as in it the lesser omentum only was involved. Unfortunately the description given is very brief. The patient was a female aged fifty. She presented deep jaundice and great emaciation. *Post mortem* the liver was found larger than normal, soft, of a uniform olive-green colour. The ducts were universally dilated, but not to an extreme degree, and they were filled with deep green limpid fluid. Gall-bladder of moderate size, and filled with the same kind of bile as that observed in the ducts. "The small omentum, from the liver to the pancreas, was infiltrated with dense, white scirrhus." The vessels and ducts passed through the midst of this mass. The former were pervious, but the latter were narrowed and compressed, so as to be completely impervious. (Path. Soc. Trans., vol. IX. [1858], p. 225).

Septimus Gibbon (Path. Soc. Trans., vol. IX. [1858], p. 214) describes a case of primary cancer of the great omentum. No details are given as to microscopic appearances.

Longmore (Path. Soc. Trans., vol. XI. [1860], p. 98) relates a case of primary tumour of the great omentum, which he tells us presented the "usual appearances of scirrho-encephaloid cancer."

Bristowe's second case (Path. Soc. Trans., vol. XVII. [1866], p. 135) presented during life a tumour in the position usually occupied by growths in the great omentum. *Post mortem* this was found to consist of the great omentum contracted and thickened and infiltrated with colloid material.

Outgrowths of colloid cancer occurred all over the peritoneal surface and in the pleuræ and parietal pericardium. The stomach was ulcerated by extension of the colloid infiltration. "The liver small, compressed by the adventitious growths which had formed around it; and its duct was compressed, and to a considerable extent obstructed by an accumulation of the same material in the substance of the lesser omentum."

Crisp (Path. Soc. Trans., vol. XXVI. [1875], p. 123) describes an instructive case under the name of cancer of the omentum. The patient was a man aged fifty-nine years. A tumour of the great omentum was distinguishable during life, and *post mortem* that structure was found to be drawn up into a hard, narrow, lobulated mass. "Behind the cæcum was a small, hard, cancerous mass of the same character." Beck and Arnott, who reported as reference committee on "Dr. Crisp's specimen of colloid cancer of the omentum," describe the tumour of the omentum as consisting of nodules more or less separate from each other, being covered with a dense fibrous membrane. Microscopically examined the growth was found "to be composed of a fibrous stroma arranged so as to form circular and oval alveoli. The alveoli in the more recent parts are filled with cells, irregular in shape and size, having one large clear nucleus. In the older parts the alveolar spaces are filled with a perfectly homogeneous colloid matter." The writers suggest an endothelial origin of the growth. "The cells have a tendency to adhere to the bands of fibrous tissue, suggesting that they might be the result of proliferation of the endothelium of the lymphatic spaces of the omentum. . . . We think it possible that the endothelium of the lymphatics may be the starting-point of the growth."

Eve (Path. Soc. Trans., vol. XXXVII. [1886], p. 496) describes a primary connective tissue tumour of the great omentum, for which myxoma or myxo-sarcoma would be, he

considers, the best name. The growths presented an alveolar structure; "each alveolus filled with a homogeneous gelatinous material, in which, in some instances, beautiful figures of minutely-branched connective tissue cells and spindle-shaped nucleated cells prolonged into slender processes are clearly visible." True colloid cancer of the omentum, Eve remarks, is commonly seen as a secondary growth, "but there is, on anatomical grounds, reason to believe that, if growths from the endothelium are excepted, primary cancer of this structure does not occur."

West's case, above referred to, may be found in the forty-first volume of the Path. Soc. Trans. (1890). "A tumour of the great omentum could be clearly made out during life. This was found to be an irregular mass about $1\frac{1}{2}$ inches thick, weighing 21 ounces. The peritoneal coat of the intestine was a good deal thickened, and in many parts at the commencement of the mesentery small, numerous, hard, whitish masses existed." There were no new growths elsewhere, so that these tumours must be considered primary. Eve and Sharkey, who examined both omental and mesenteric growths, briefly describe their structure as "scirrhus carcinoma."

The accompanying plate gives a fairly good notion of the naked eye appearance of the tumour in my case. At A the left lobe of the liver is shown drawn up somewhat so as to indicate that it is not attached to the new growth. B points to the under surface of the posterior portion of the same lobe. C is the cut edge of the falciform ligament. D, right lobe of liver. E, peritoneal connection of tumour with stomach. F, attachment to large intestine. G points to the most convex anterior portion of tumour. The depression where the peritoneal connection existed with the colon is represented in the plate by much too deep a groove, and thereby the growth is given a more distinctly lobulated appearance than it really presented.

ON THE RARER FORMS OF FRACTURE OF THE CARPAL EXTREMITY OF THE RADIUS.

By E. H. BENNETT, M.D., F.R.C.S.;

Professor of Surgery in the University of Dublin.

[Read in the Pathological Section, February 26, 1892.]

THE list of fractures of the lower end of the radius runs thus:—

1. Colles' fracture (three varieties).
2. Colles' fracture reversed.
3. Epiphysary separation.
4. Rhea Barton's fracture.
5. Rhea Barton's fracture reversed.
6. Fracture of the radial styloid.
7. Longitudinal fracture or fissure of the lower end of the bone.

I propose to submit very briefly some examples which illustrate the pathology of the second and last of these groups. I have already published^a my views with regard to Colles' fracture, and I think established the features of its three varieties by the detailed examination of an ample pathological series, which numbers over a hundred examples. I shall refer to these varieties only so far as is unavoidable in the discussion of the reversed Colles' and the longitudinal fractures contained in No. 7. Epiphysary separations have had their features so fully and clearly delineated by Professor R. W. Smith that I need not speak of them.

4 and 5—Rhea Barton's fracture and its reverse—are now admitted to be only Colles' fracture, and the groups are of interest only as they serve to record the fact that this observer as long ago as 1838 recognised the clinical features

^a British Medical Journal. May 22nd, 1880.

of the lesion which modern American writers attribute to Smith as its describer—that is, reversed Colles’.

The view Rhea Barton held of these injuries was that in the first and more common injury extreme extension of the wrist pressed the carpal bones so heavily against the dorsal margin of the carpal articular surface of the radius that this margin broke off.

In the rarer injury overflexion of the wrist pressed, according to the author, the carpus so hardly against the anterior border of the radial carpal surface as to carry it away.

One of the latest American writers^a on the subject disposes of this fracture thus:—“In the forty-five years which have elapsed since the publication of these views there has not been, so far as I have been able to ascertain, a single instance placed on record in which they have been confirmed by dissection.” I can fully endorse this statement, and should not have placed the groups on my list but for the fact that the text-books of our American fellow-workers in general include them, and for the fact that, though in error in his pathological detail, it is clear that Barton was amongst the first observers of his time; that he recognised Colles’ fracture and its reverse as distinguished from dislocation of the wrist. I may therefore pass this section of the subject by stating only that I can produce many specimens which might, at the first glance, be put down as Barton’s fracture, but they are all reducible to fractures of the carpal surface secondary to the occurrence of impaction in Colles’ fracture.

I now submit a few facts as to the reversed Colles’ fracture, which would probably have remained for some time unpublished but that the chance of daily surgical practice has enabled me to record a case observed in the living, a tangible record of which I have preserved in this cast.

Here is Smith’s cast of the injury, which is his only fact

^a Packard Ashurst’s *Encyclopædia*. Vol. IV., p. 168.

and it is clear to anyone who reads the description that the history of the case was not in his possession, for he gives us no details regarding it. He says:^a—"I cannot speak with accuracy as to the anatomical characters of the injury, having never had the opportunity of examining after death the skeleton of the forearm in those who had during life met with this accident, nor is there any preparation showing the exact relative position of the fragments in any of the pathological collections in Dublin."

Hamilton, in his sixth edition, 1880, refers to the rarity of this fracture, and quotes part of the passage I have just read in support of this view. Mr. Callender's two specimens of this injury and a single clinical observation seem to be the total of London cases.

Professor Gordon^b published a woodcut representing the radius broken in this manner, and states that there exists a second pathological specimen of the fracture in the Museum of the Queen's College, Belfast; but he says, "I have not met with the accident in the living." Yet on this slender stock of facts he names the injury the "articular fracture of the lower end of the radius"—a name most inappropriate, as it would lead a novice to suppose that this alone of fractures of the lower end of the radius involved the radio-carpal joint.

Of the eight specimens of this fracture before the Academy, three present their carpal surfaces unbroken, and five have these surfaces more or less fissured by fractures. I think that these comminutions have resulted from the same cause as that which produces the comminutions of the lower fragment in ordinary Colles' fracture—namely, the intrusion of the lower end of the upper fragment into the lower, which causes a greater or lesser degree of splintering of the lower. The fact that three of these specimens and the example

^a On Fractures in the Vicinity of Joints. R. W. Smith. 1847. P. 163.

^b Fractures of the Lower End of the Radius. 1875. P. 30.

figured by Voillemier^a are fractures which do not involve the wrist-joint disposes of Prof. Gordon's name for the injury—"the articular fracture." In all these examples the radius is broken transversely as in Colles' fracture; but the lower fragments in one piece or comminuted, is displaced forwards.

The injury recorded by this cast was the following:—A jockey, aged nineteen, was backing for the first time a three-year-old filly, she bucked and burst the girths, and sent saddle and rider into the air. The rider fell to the ground, striking first on the back of his right hand. I saw him the day following. The injury had been readily recognised by the deformity, the facility of reduction, and the distinct osseous crepitus felt on the reduction; immediately on the relaxation of the reducing force the deformity recurred. I took the patient into hospital and verified the details, and treated the patient until union was obtained.

It would seem then that Rhea Barton and Mr. Callender were hitherto the only observers of the clinical details of the injury, and that the pathological facts bearing on it were not much more plentiful here or in America. I need not then apologise when I submit a cast of the recent injury taken previous to reduction, and a series of eight pathological specimens obtained—one from the Museum of this College, to which it was presented by Mr. Swan, in 1885, and the seven which I have myself procured from the anatomical department of the School of Physic in Ireland. I think we may then fairly reject the conclusion arrived at by M. Lecomte, who states his opinions in a series of dogmatic assertions, of which this is the first:—"All the fractures of the inferior extremity of the radius are produced by a single and the same class of fall; falls on the palm of the hand."

It is, I think, clear that the reversed Colles' is the result of a fall on the back of the hand.

^a Clinique Chirurgicale. Pl. I., Fig. 1.

I shall now briefly examine the longitudinal fractures of the lower end of the radius, and I hope show that they result from causes other than falls on the palm of the hand. The leading case which bears on this question is that recorded by Bigelow. The Catalogue of the Warren Museum contains this description of the specimen, 1035:—"The lower extremity of the radius showing a stellate crack of the articular surface and extending upwards for more than an inch. From a patient who died of other injuries. At first there was only a complaint of lameness as from a sprain, but after some days there were swelling and tenderness, which, from their persistence, led Dr. B. to diagnosticate a stellate crack of the bone as a probable result of the fall, and of which he had met with a similar case two years previously. Dr. B. remarked that the bones of the wrist would act as a wedge upon the concave articular surface of the radius; and that this case would explain the persistence of some cases of sprained wrist." Case published in the *Medical Journal* (Vol. LVIII., p. 99) with a figure, and copied with the figure in Dr. F. H. Hamilton's works on Fractures, 1848.



Fig. 1.

The details of the injury given here are very meagre, and leave us in doubt as to whether the patient fell on the palm of the hand or not. From the description and figure it appears that there was not any transverse fracture of the lower end as in Colles' fracture, and that the fissures were not, therefore, such as occur in the articular surface in at least one-half of the cases of Colles' fracture.

I now submit a series of radii which belong, two to the Museum of the College of Surgeons, and five to the Museum of the School of Physic, which present fissures that occurred during the life of their owners; with these I submit two broken experimentally. The earlier specimens that I obtained showed fissures of the articular surface only, and occupied my attention because of the difficulty in explaining their origin. This specimen (Fig. 2), placed in the Museum of



Fig. 2.

this College by the late Dr. Richardson, was the first which I saw that helped me to a solution of the question; it was damaged by being caught in machinery, probably a cog-wheel—such is the note on the label.

I at once tried if I could produce such fractures on the

dead bone. If one wraps a towel round the end of the dry bone, and presses with the heel on the carpal end of the radius as it lies on the ground, the bone yields and crepitus is felt; with the lighter degrees of pressure it is possible to break the bone only in the concave surface of the articulation, and to produce lines identical with those of these macerated specimens, strictly limited to the articular surface (Figs. 3, 4, 5, 6). A greater degree of force extends the fissures upwards in the compact wall of the bone. If a man be trampled in a crowd, or if a weight of any kind falls on the wrist, such a damage can readily happen, still more if a



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.

wrist, as we often see, be rolled in machinery. It seems more rational to look for the cause of these fissures in such injuries than to attribute them to the wedge action of the carpal bones.

The experiment I have described is best made on bones in which maceration has removed the articular cartilage; in these the fissures of least degree will be seen in the articular layer of bone, but would be concealed in the bone covered by cartilage. This is illustrated by one of my specimens, where the cracks are evident in the shaft on both palmar and dorsal sides of the bone, and are traceable into the cartilage at each margin of the joint, but have not torn it.

Of the fracture of the radial styloid I can only say that this specimen (Fig. 7) is a perfect example, remarkable for



Fig. 7.

the downward displacement of the fragment, which has caused elongation of the outer border of the bone, probably as we see the inner malleolus displaced by traction of the internal lateral ligament of the ankle joint. I have never recognised the injury in the living, and this united fracture is the only example I have seen.

SYPHILITIC GUMMATA IN LIVER AND SPLEEN.

By WALTER G. SMITH, M.D.;

Physician to Sir Patrick Dun's Hospital;
Professor of Materia Medica, School of Physic, T.C.D.

[Exhibited before the Section of Pathology, February 26, 1892.]

DR. WALTER SMITH exhibited specimens taken from a man, aged sixty-three, admitted into hospital January 2nd, 1892; died January 9th, 1892. Nine years previously he had contracted chancre, followed by secondaries, and, later on, by ulcers on legs and forehead. The soft palate was deeply scarred. Six months before admission to hospital he noticed that his strength was failing. He was thirsty, constipated, and passed a large quantity of urine. While in hospital the urine was acid, low Sp. Gr. (1009), and contained a little albumen; no sugar. The lungs and heart presented no evidence of disease; slight œdema of legs. He died rather suddenly a week after admission.

At the autopsy the only noteworthy point was the occurrence of gummatous nodules in liver, spleen, and kidneys. The hepatic tumours were on the under surface of liver, two or three in number, largest about 1 inch in diameter, laminated, and yellow on section. The nodules in spleen were small and spherical. One or two small nodules in periphery of kidney.

Microscopical examination of the nodules by Dr. H. C. Earl showed the usual characters of syphilitic gummata.

It may be remarked that as regards visceral syphilis, gummata of the kidney and spleen are rare. The first specimen of gumma of the spleen exhibited in London was shown by Dr. Wilks in 1860. (Trans. Path. Soc. Lond. Vol. XII.)

PAINLESS CANCER OF THE LIVER.

By JAMES LITTLE, M.D., F.R.C.P.;

Physician, Adelaide Hospital;
Ex-President, Royal College of Physicians.

[Read in the Section of Pathology, April 1, 1892.]

DR. LITTLE showed a liver weighing 178 ounces. The patient, a man forty-eight years old, had been under Dr. Little's observation for a month before his death. The whole duration of his illness was only four months. The early symptoms had been chiefly symptoms which suggested disease in the stomach—namely, loss of appetite, dryness of the mouth, waterbrash, and regurgitation of food; in addition, from the beginning of his illness, the patient had suffered from drowsiness and constipation, and at the time when he came under Dr. Little's observation he was deeply jaundiced, and subsequently there was considerable effusion into the peritoneal sac.

At the *post-mortem* examination were found, in addition to a liver which constituted a beautiful specimen of the brain-like cancer, cancerous nodules in the lesser omentum, cancerous nodules in both lungs and enlargement of the mediastinal glands, and, of more clinical significance, a hard growth and a deep ulcer in the rectum. Though the patient had never complained of symptoms referable to the rectum, Dr. Little considered that the rectum had been the original seat of the cancerous growth, and the growths in the liver and other organs had been secondary. Cancer of the stomach was the most common starting-point of cancer of the liver, but in this case the stomach was unaffected. Dr. Murchison had ranged cancer of the liver among the painful enlarge-

ments of the liver; but in this, and in many other cases of cancer of the liver which had come under Dr. Little's observation, there was little pain, and the patient in the present case never complained of pain even when his abdomen was being examined by the class.

Dr. H. T. Bewley had examined microscopically some of the enlarged glands from the lesser omentum. The sections showed a mass of cancer, consisting of loculi, generally elongated in shape, and made of not very dense fibrous tissue, which were filled with polygonal epithelium-cells. In places there were attempts at the formation of tubular glands, but in most cases the elongated masses of cells were solid. Dr. Bewley called attention to the fact that cancers secondary to those of the intestine generally reproduce the tubular glands of the intestine. In this case the epithelium was polygonal in shape, and formed solid cylinders and masses. This peculiarity, he thought, was due to the very rapid growth of the cancer—the growth being even more atypical than usual. The peculiarity of the growth chiefly lay in the fact that although it was excessively hard to the touch, and therefore gave rise to the suspicion that it would prove to be a scirrhus, yet the microscopic structure was that of typical adeno-carcinoma—well-formed gland tubes with comparatively little stroma, and lined by high columnar cells, sometimes lying in several strata within the neoplastic tubule.

HUMAN AND FOWL TUBERCULOSIS.

By ALFRED R. PARSONS, M.B. UNIV. DUBL.;

Assistant Physician to Sir P. Dun's Hospital.

[Read in the Section of Pathology, April 1, 1892.]

AN investigation into the causation of any given disease may be undertaken from three different standpoints. The clinical history and bedside phenomena of several similar cases, grouped and compared, may form the basis of a theory as to the probable nature of the affection, or the anatomical lesions and their distribution may present indications of ætiological significance; or, finally, experimental pathology may indisputably point to the infective nature of the process, and thereby render sure the first step towards its complete elucidation. Centuries of clinical observation threw but little light on the causation of tuberculosis. If certain cases appeared inexplicable on any but an infective theory, others presented themselves in which no such assumption seemed admissible. Pathological anatomy, in the hands of Buhl, Ponfick, and Weigert, explained the occurrence of miliary tuberculosis by a systemic infection from an old caseous focus, but could give no information regarding the causation of the latter. Experimental pathology, nearly sixty years ago, first demonstrated successfully the infective nature of the tubercular process when Klincke in 1843 produced extensive disease of the lungs and livers of rabbits by inoculating them with portions of organs containing numerous tubercles. These researches, soon forgotten, were confirmed and extended some twenty years later by Villemin, who produced artificial tuberculosis, and proved experimentally the identity of the human and bovine forms. His results excited con-

siderable attention, and the opponents of the infective theory claimed to produce tubercles by the inoculation of non-tubercular matter. To Cohnheim and Salomonsen is due the credit of demonstrating, by a method beyond suspicion, the infective nature of tuberculosis. In a happy moment they thought of selecting the anterior chamber of the rabbit's eye for their experiments, and proved conclusively that the minute tubercles in the iris, characterised by their late development, further course and caseation, were produced only after the injection of tubercular matter.

So much was known of the causation of tuberculosis when Koch commenced his classical researches. These culminated in a paper read by him on the 24th March, 1882—just ten years ago—before the Physiological Society of Berlin, in which he claimed to have discovered the cause of tuberculosis in the form of a well-defined bacillus. A decade of this critical and sceptical age has since passed away, Koch's claims have been investigated by friends and foes, and to-day every line of that original treatise, save a few statements of comparatively slight importance, remains intact. One of these statements—the error of which Koch himself has recently pointed out—dealt with the relation of avian to mammalian tuberculosis, and as a further series of investigations on this subject have been lately made by two members of the French school, I thought it would not be amiss to lay a summary of the more recent literature of this question before the Academy.

Of the many difficulties against which Koch had to contend in his earlier experiments, none probably exceeded that of obtaining a pure culture of the tubercle bacilli. After many failures he at length succeeded in growing them on blood-serum. In 1885 Nocard pointed out that the addition of small quantities of sugar and peptone to the serum favoured their development, and a little later Nocard and Roux showed

that if glycerine be added to other of the nutritive media, the tubercle bacilli appeared to grow more easily, to change their pathological properties, and to increase their virulence. Yersin obtained some of these cultures on glycerine-agar, and found that by inoculating them into the veins of a rabbit it was possible to produce a form of tuberculosis characterised by the absence of tubercles. The inference drawn from such observations, and one which gained currency in France, was that by cultivating the tubercle bacilli on glycerine-agar, it was possible so to alter their properties as practically to produce a new variety of that organism. This inference was apparently supported by an observation of Metschnikoff, who pointed out that tubercle bacilli, developed on agar-glycerine, grew luxuriantly at $43.6^{\circ}\text{C}.$; whereas Koch had stated that growth ceased if the temperature exceeded $41^{\circ}\text{C}.$

To a member of the Italian school belongs the honour of first suggesting what eventually proved to be the correct explanation of these discordant results. Rivolta, in 1889, expressed the suspicion that the bacilli found in avian and in mammalian tuberculosis belonged to two distinct species. Experiments conducted by Maffucci and others, confirmed this suspicion, and Koch, in his paper on "Bacteriological Investigation," read before the Tenth International Medical Congress in 1890 at Berlin, cleared up still further the disputed matter. He stated that when making his original investigations, the tubercle bacilli from the most different sources not only presented the same microscopical appearance, and reacted to the same stains, but gave cultures indistinguishable from each other. From fowl suffering with tuberculosis, owing to his inability at that time to obtain fresh material, he was unable to produce any cultures; but, finding that the bacilli in their appearance and in their reaction towards the aniline dyes agreed fully with those of which he had made cultures, he felt justified in looking upon

them as identical. Some time later he received from experimenters, whose veracity and ability were above suspicion, cultures stated to be pure cultures of tubercle bacilli, which differed considerably from those he himself had made, and producing on inoculation results quite distinct from those he had described. The first explanation which presented itself to Koch's mind was that these bacilli had been developed under unfavourable conditions, and consequently he commenced a series of experiments in which he watched the growth of his own bacillus at unusually high temperatures, and strove by adding chemical substances to his cultures, by varying the amount of moisture, by developing the bacilli along with other micro-organisms, by inoculating them on a number of slightly sensitive animals to produce growths identical with these puzzling forms. In these endeavours he completely failed, and the problem was not solved till he accidentally obtained some living fowl affected with tuberculosis. From these he made some cultures, and to his amazement found them to correspond exactly with those enigmatical varieties which had been sent to him, and consequently he now holds that avian tubercle bacilli, though closely related to human tubercle bacilli, constitute a distinct variety.

A very exhaustive and highly interesting series of experiments have been recently made on this subject by MM. Straus and Gamaleïa. By cultures and inoculations they have indisputably demonstrated the difference between the bacilli causing avian and those causing mammalian tuberculosis. Proceeding exactly by the method indicated by Koch, they injected some phthisical sputum into a guinea pig, and a fortnight later inoculated a series of tubes containing blood serum with portions of its enlarged glands. The tubes were kept at a temperature of 37.5° C., and after a lapse of two or three weeks minute, dry, scaly grains of a dull whitish colour, of firm consistence and possessing considerable

cohesion made their appearance. Tubes of serum inoculated with portions of the organs of tubercular fowl presented, on the contrary, round, whitish, moist spots, softer in consistence, and with much less cohesion than those of human source. If these growths were transferred to tubes of glycerine agar the difference was quite as marked. Gradually in each case the agar was covered over with a continuous whitish coating, which was surmounted in human tuberculosis with numerous warty elevations, but in fowl tuberculosis was provided with wrinkles and furrows. I think these coloured plates which illustrate Straus and Gamaleïa's paper make these distinctions quite evident, and I am happy, through the kindness of Professor Straus, to be in a position to submit to the Pathological Section of the Royal Academy of Medicine some of his pure cultures of human and of fowl tubercle bacilli on this the first occasion on which pure cultures of these micro-organisms have been exhibited to the Academy. Straus and Gamaleïa ascertained that the avian bacilli developed luxuriantly at 43° C., while the human bacilli ceased to grow if the temperature exceeded 41° C. They further obtained cultures of tubercle bacilli from a large number of the French laboratories, and found them invariably to correspond with those of fowl tuberculosis. Consequently nearly all the literature of the French school on the bacteriological aspect of tuberculosis has been based on experiments made with fowl bacilli, and may or may not be true of mammalian tuberculosis.

The characteristic lesions produced by the inoculation of these cultures on animals were even more striking than the differences in the cultures themselves. They submitted some 82 guinea pigs to experimental investigation. Of these 56 were inoculated with human tubercle bacilli, and 26 with fowl bacilli. Whether the method adopted was sub-cutaneous, intra-peritoneal, intra-pulmonic, or intra-venous,

they found invariably that the animals inoculated with human bacilli presented at death a generalised eruption of macroscopically visible tubercles, hypertrophied caseous glands, and an enlarged yellow spleen; but if the bacilli were of an avian source there was no eruption of macroscopically visible tubercles; the bacilli were occasionally found in the organs, but the most constant lesion was a swollen red spleen. They experimented similarly on rabbits, and found that the usual pathological appearance in those animals injected with human bacilli was an eruption of tubercles, particularly abundant in the lungs, while the animals treated with fowl bacilli likewise succumbed, but presented no tubercles visible to the naked eye.

At one time an idea was prevalent that it was exceedingly difficult to produce artificial tuberculosis in dogs, in fact, more than one experimenter wrote that he knew no means by which an adult dog could be infected with tuberculosis, and consequently it was attempted by the injection of dogs serum to render a rabbit immune to tubercular infection. Straus and Gamaleïa experimented on 15 dogs. They found, as Maffuci and Koch had previously shown, that an injection of a small quantity, say $\frac{1}{4}$ c. c. of an emulsion of human tubercle bacilli made into a dog killed him in one or two months, and at the *post-mortem* examination the lungs were found studded with miliary tubercles. On the other hand, comparatively large quantities of an emulsion of avian bacilli could be injected without producing any appreciable effect. True, if one used 20–30 c. cs. of a thick emulsion the dog wasted and died at the end of a month or six weeks, but no tubercles were visible at the autopsy, and death appeared to be due to a special intoxication.

Dogs are, therefore, excellent media for determining whether we are dealing with a culture of human or of fowl tubercle bacilli. In this, however, they are left far behind

by fowl themselves. The difficulty, indeed, of infecting fowl with phthisical sputum has been often pointed out. Suffice it to recall the experiments of Straus and Wurtz, who fed eight hens for periods varying from six months to one year on expectoration containing numerous bacilli, and at the end of that time, though some of the fowl had consumed upwards of 45 kilogrammes of sputum, when they were killed they presented no tubercular lesion. Straus and Gamaleïa's researches were made on twenty pigeon and fowl. They injected considerable quantities of pure cultures of human tubercle bacilli under the skin, into the pectoral muscle, into the axillary vein, into the peritoneal cavity of these animals without producing any appreciable result; but an injection of a small quantity of avian tubercle bacilli produced rapid wasting and death at the end of a few weeks or months. At the *post-mortem* examination abundant bacilli were found in the organs, and sometimes an eruption of generalised tubercles. In fowl, therefore, we have animals absolutely and completely refractory to human tubercle bacilli, but succumbing easily to their own variety.

Out of these researches arises a very important question: "Are men susceptible to fowl tubercle bacilli?" To that question no direct answer can be given at present. So far as I am aware, in no case has a culture presenting features identical with those of fowl tuberculosis been obtained from a human subject, but as the distinction between the two varieties is of comparatively recent date, a sufficient number of experiments have probably not yet been made to justify a reply in the negative.

Of the prevalence and destructive tendency of human tuberculosis our hospitals and the returns of the Registrar-General afford convincing proof. Similarly, among the affections from which fowl suffer there is none more dangerous than tuberculosis. Should this disease appear in a farm-yard

the only effective method of stamping it out is to kill off all the members of that variety which has been attacked. Occasionally no less severe measures than the destruction of all the birds will rid the farmyard of this scourge. Farmers have, indeed, learned by experience the infective nature of avian tuberculosis, and recognised that they must resort to radical means to prevent a local outbreak becoming general, but the public are practically oblivious to the great risk of infection to which they expose themselves, while the phthisical portion of the community scatters broadcast sputa containing organisms remarkable for their resisting power to agencies generally destructive to the lower forms of life.

Men are most commonly infected by the inhalation of the dried expectoration of their fellow creatures; but from the distribution of the anatomical lesions in fowl it seems probable that infection occurs through the intestinal rather than the respiratory tract. An examination of the mucous surface of the former in a fowl which has succumbed to an attack of tuberculosis discloses numerous ulcers with infiltrated edges, while nodules of varying size are often found in or attached to the intestinal wall. These sometimes undergo caseous degeneration, discharge their contents into the tube, and produce a pouch or diverticulum communicating freely with lumen of gut. Thus a large quantity of cheesy material containing numerous tubercle bacilli is discharged per anum and many foci of infection established. The appearance of the liver is even more characteristic. Scattered with some degree of uniformity over its surface are multiple whitish yellow patches. These differ considerably in size—some appear no larger than a pin's head, others have an area equal to that of a sixpence. They extend also for varying depths into the substance of the liver, and seem to be as numerous in the deep as in the superficial parts. Notwithstanding the number and size of these new growths jaundice is an exceed-

ingly rare complication. Out of several hundred birds examined by Mr. Bland Sutton^a in only one was there any general deposition of bile pigment in the tissues. Microscopical examination shows that these masses consist generally of a central caseous area surrounded by giant cells, with many nuclei, in each of which the nucleolus is visible. Beyond these lies a zone of granulation tissue, which in the larger nodules has gone on to the formation of fibrous tissue, separating them sharply from the surrounding liver substance. The latter is occasionally infiltrated with small round cells, and is sometimes in a condition of amyloid degeneration. The bacilli may be found in the central caseous area, but are present more commonly and abundantly in the giant cells and granulation tissue. With the exception of the spleen, which contains nodules similar in appearance to those found in the liver, the remaining abdominal viscera are, as a rule, free from disease. In a few cases out of a large number examined by Mr. Sibley^b there was a slight deposit at the hilum of the kidney or ovary. Though considerably over 1,000 birds have been examined by the above-mentioned observers, in only two cases did they find any deposit in the lungs visible to the naked eye. The lymphatic glands in the neck are by no means infrequently engaged; they undergo caseous degeneration, but do not suppurate or discharge externally. Mr. Sibley met with two cases presenting caries of the vertebral column, and one with hip-joint disease and an abscess cavity in the upper part of the thigh bone.

In the preparation of fowl for the market it sometimes happens that the internal organs are found to be in the above condition. This does not, however, so far as I have ascertained from those engaged in the work of preparation, in their opinion injuriously affect the bird, or render it in any

^a Trans. Lond. Path. Society. Vol. XXXV. P. 477.

^b Trans. Lond. Path. Soc. Vol. XXXIX. P. 464.

way unfit for food, provided the disease be confined to the viscera. The safety of this practice may possibly be questioned, while it must be admitted that deposits in the joints are rare, and no observer seems to have detected them in the muscles.

In conclusion, let me say that the earlier part of the bacteriological summary is derived chiefly from one of Koch's original reports on tuberculosis (*Mittheilungen aus dem Gesundheitsamte*. Vol. II. 1884. Translated by Boyd in selected essays), while to Straus and Gamaleïa's highly instructive article in the *Archives de Médecine Experimentale* I am indebted for the greater portion of it.

RHINOSCLEROMA.

By W. G. T. STORY. M.B.;

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[Read in the Section of Pathology, April 1, 1892.]

THE disease known as Rhinoscleroma was first described by Hebra in 1870. In the cases seen by him the nose and adjacent parts, more especially round the external orifices of the nose, were swollen and infiltrated and very hard to the touch, and it was owing to this feature that the name Rhinoscleroma was given to it by Hebra, who supposed that it was of a sarcomatous nature. Since then, however, numerous other cases have been seen and described in Austria, Russia, Germany, Italy, and in Central America; and it has been found that the nose is not always either the starting-point or the seat of the disease.

The usual course of the disease is as follows:—After a history of long-standing nasal catarrh, accompanied in some cases by a tendency to bleeding, a chronic hypertrophic inflammation of the mucous membrane is observed; this generally commences at the choanæ, and may spread thence towards the anterior nares, the lips and gums, causing all these parts to become swollen and hard, or may spread downwards to the soft palate, pillars of the fauces, pharynx and larynx.

The parts attacked become greatly swollen, darker in colour than the surrounding healthy mucous membrane, and very hard to the touch; after a time nodules, also very hard and some of them as large as a hazel nut, develop on the surface of the swelling, and on these or between them superficial loss of substance may occur, but deep ulceration

is almost never seen. The swellings are not painful, but by their size may cause in the nose difficulty of breathing, loss of smell, and blocking of the Eustachian tubes, with consequent deafness and tinnitus; in the lips, mouth, and pharynx difficulties in mastication and deglutition, and in the larynx persistent cough, aphonia, and attacks of dyspnœa, which are the more frequent, as the swellings give off a very sticky secretion which tends to form plugs and crusts. This is the stage of infiltration; it is followed by one of cicatricial contraction, when the swellings disappear and are replaced by cicatricial tissue, at first also very hard but becoming later more fibrous and greatly inclined to contract. This contraction may also cause severe symptoms, more especially stenosis of the larynx and trachea; like the swellings, the cicatricial tissue is covered with smooth thickened epithelium. The conversion into cicatricial tissue commences in the parts first attacked, so that in an old case it is often found to have taken place in the more central parts, while fresh nodules of infiltration are still appearing at the periphery. The disease is very chronic in its course; it is generally seen in young adults, though cases have been observed in children and in old persons; except indirectly, it does not seem to affect the general health.

Medicinal treatment has been of no avail, and what would seem the most rational method of cure—that is to say, complete removal of the diseased tissues—is so difficult to carry out that I do not know that there has been any quite successful case published. The diseased tissue, though so hard, is surprisingly easy to cut, and the wounds made heal very readily; but it is very hard to get away all the diseased parts, and foci of infection are generally left from which the process may start afresh. Treatment is therefore, to a large extent, palliative: the most important complica-

tion—stenosis of the glottis or trachea—being treated by dilatation with bougies or by tracheotomy.

The pathological changes consist in an infiltration of the mucous and submucous tissues with small round cells and spindle cells. Among these are to be found a number of large round cells, which have a blown-out appearance, and are named after the observer who first described them—the cells of Mikulicz; the papillæ are enlarged and the epithelium thickened. In old diseased parts the small-celled infiltration is replaced by dense connective tissue, and the cells of Mikulicz are very scarce, but small masses of so-called hyalin material, very retentive of aniline stains, are to be found, and these are believed by most observers to be a degeneration product of the cells of Mikulicz.

In 1882 Frisch discovered that small bacilli were nearly always present in the cells of Mikulicz and also lying outside them; he was able to make cultivations of these bacilli, but his inoculation experiments were without result. Later, Cornil and Alvarez, and after them Paltauf, and v. Eiselsberg in Vienna, noticed that when stained in certain ways each bacillus might be seen to be surrounded by a capsule of what they supposed to be a colloid material. For instance, when carbol fuchsin was the stain used, the capsules were visible, but when Löffler's methylene blue was used the capsules were unstained, and the bacilli looked therefore much smaller. The bacilli are for the most part contained in the cells of Mikulicz; in these cells a nucleus may often be seen lying at the periphery, as if it had been pushed to one side by the contents of the cell. These bacilli were successfully cultivated in six cases of typical rhinoscleroma by Dr. Paltauf in 1885. The cases were typical of the disease as described by Hebra, as the nose and external nasal orifices were involved; drops of blood or exudation were taken from cuts made in the tumours with the usual

precautions, and pure cultures were obtained on gelatine plates and tubes, on agar-agar and potato. On the plates they appeared, after a couple of days, as small round white colonies as large as the head of a pin, and in gelatine tubes in which "Stich-kulturen" were made they took the form of a nail—in fact, were typical "Nagelkulturen;" they did not liquefy the gelatine. On agar-agar the colonies appeared more rapidly as white translucent masses, about 5 mm. in breadth, and on potato they appeared as a cream-like layer, which spread gradually from the point of inoculation. Under the microscope the cultures were found to be made up of short rod-like bacilli with capsules which could be stained, and of cocci, chiefly diplococci, also with capsules. These micro-organisms resembled, therefore, exactly those seen in the tissues. They resembled also in a remarkable way, both in their method of growth and in their appearance, the bacilli which had been described by Friedländer as the cause of acute croupous pneumonia.

With these pure cultures inoculation experiments were made, and control inoculations were made with Friedländer's bacillus. The results in each case were very similar—that is to say, that in mice and guinea-pigs inflammation of the pleura and death could be caused, and also local abscesses in the subcutaneous and muscular tissues, but no process resembling rhinoscleroma in man. The rhinoscleroma bacilli were markedly less virulent than Friedländer's bacillus. Rabbits were not affected by either bacillus.

Last year Drs. Paltauf and v. Eiselsberg published 15 more cases in which they have successfully made pure cultures of the bacilli; this makes 42 published cases in which this has been done, and this large number—considering the small number of cases of the disease observed, that in all the cases tried cultivations could be made, and that the bacillus could be always found in hardened sections—may be considered

conclusive evidence that the micro-organism is the cause of the disease.

Of these last 15 cases only one was a typical case of the rhinoscleroma of Hebra—that is to say, with hardening of the external parts of the nose, infiltration of the alæ nasi, and swellings of the external nasal orifices without ulceration.

In 2 of the cases there was ulceration, which is rare in rhinoscleroma, and in both these it was believed that the ulceration was, if not caused, at least rendered worse by the antisyphilitic treatment, both local and constitutional, to which, owing to mistaken diagnosis, the patients were subjected.

In 5 of the cases the disease never spread to the external orifices of the nose, though the nasal mucous membrane was attacked.

In 7 cases the disease was primary in the larynx or trachea, appearing there as the chorditis vocalis inferior hypertrophica described by Gerhardt; in 3 of these, bacilli in the cells of Mikulicz were found; in the other 4 and in 5 other cases where the larynx was involved, no tissue could be got for microscopical examination, though the clinical observations left no doubt as to the nature of the disease.

In 3 cases of suspected rhinoscleroma cultivation experiments gave a negative result, which was confirmed by events—the tumours disappearing under antisyphilitic treatment in one case, and in the others turning out to be of a tubercular and of a carcinomatous nature respectively. Dr. Paltauf concludes, therefore, that cultivation experiments are of great diagnostic value, and in doubtful cases conclusive for or against the nature of the disease.

As to the relationship of the bacillus of rhinoscleroma to that described by Friedländer as the bacillus of croupous pneumonia, it was found that there were points of difference in the cultures:—

1. In tubes containing 1 per cent. of grape sugar, Friedländer's bacillus caused fermentation and complete disappearance of the sugar in 12 days, whereas the bacillus of rhinoscleroma only caused 66 per cent. of the sugar to disappear.

2. Friedländer's bacillus will grow in an acid medium; the other will not.

3. Friedländer's bacillus will coagulate milk, whereas that of rhinoscleroma will not do so.

The bacilli, therefore, though closely allied, are not the same.

I should mention that Dr. Paltauf follows v. Ganghofer in giving up the term rhinoscleroma, as the disease has been shown not to be confined to the nose, and uses instead the expression scleroma of the throat, larynx or nose.

The sections on the table were given to me in Dr. Paltauf's laboratory. The first was stained in hæmotoxylin (Grenacher's) and in it the capsules surrounding the bacilli are so deeply stained that they are not transparent, and the bacilli therefore look large and are easily seen lying inside the large spheroidal cells of Mikulicz.

Under the second microscope is a section from the same specimen stained in carbol fuchsin. In this section the bacilli lying also in the cells of Mikulicz can be seen, but they look smaller: the reason of this is that the capsule is so faintly stained as to be almost invisible, and the small bacilli are seen lying inside it.

These large bacilli-holding cells are quite typical of the disease; they are not, as far as I know, seen in any other condition.

A CASE OF CHRONIC TUBERCULOUS SPINAL PACHY-MENINGITIS.

By HENRY T. BEWLEY, M.D.; F.R.C.P.;

Assistant Physician to the Adelaide Hospital, and Lecturer on
Medical Jurisprudence in Trinity College, Dublin.

[Read in the Section of Pathology, May 13, 1892.]

R. S., a draper's assistant, aged forty-one years, had always been perfectly healthy. He had never had syphilis, nor taken alcohol to excess. In April, 1887, he began to suffer from pain, worst at night, which he referred to the region of the third and fourth lumbar vertebræ. This pain did not prevent him attending to business as usual. Early in July, 1877, the pain having continued unchanged, he began to find he had not proper feeling in his feet, a hard floor feeling like a soft carpet, and to experience difficulty in walking.

On July 12th he was admitted under my care. I found that he could walk pretty well with the help of a stick. His legs appeared to be as strong as they ever had been, so that when he resisted, I could not flex or extend his joints by any force I could use. There was, however, great inco-ordination of the legs. He could stand well with his eyes open, but when they were shut he swayed about, and would fall if not held. With his eyes shut he could not take more than one or two steps without falling. When he lay on his back with his eyes shut, and tried to put his feet in any given position—for instance, to touch with the left heel the right toes—he would raise the limb about a foot too high, and bring it down beside the other, always making four or five attempts before he gradually attained the desired position. Sensation was diminished, but not lost, from the waist down. The deep reflexes in the legs were much increased, the knee jerks being excessive, and ankle clonus present. There was no wasting of muscles. The spinal column was carefully examined, and appeared perfectly normal. From the waist upwards his nervous system was perfectly healthy; the abdominal, thoracic, and cephalic viscera were healthy. He was spare and thin, but looked healthy.

He grew rapidly worse. At first, after admission to hospital,

the inco-ordination increased somewhat, but soon the excessive reflexes merged into a condition of permanent rigidity, so that in September, 1887, his legs were so stiff he could not walk.

In December, 1887, the legs were extremely rigid, so that he could not move them at all; they were extended at all the joints, and strongly adducted, and pressed one against the other. The reflexes continued excessive; there was no muscular wasting. Sensation had grown worse, so that a slight touch could not be felt. The pain in his back had ceased, but at times he complained of a feeling of tightness round his waist. He often suffered from sudden violent pains darting down his legs. Occasionally he had to have a catheter passed, but generally he had no trouble with either bladder or rectum. In the summer of 1888 the legs, from being extended, became acutely bent at all the joints. With this exception he remained in just the same state till May, 1890, when he found his back was becoming bent, a prominence forming in the lower dorsal region.

He had left hospital in June 1888, but returned to it in November, 1891. I then found that the legs had become perfectly stiff in the flexed position, passive motion being quite impossible. Adhesions had evidently occurred in the joints. His back presented a well-marked angular curvature, the ninth, tenth, and eleventh dorsal spines being very prominent. Sensation was very much impaired below the level of the twelfth dorsal nerve, but was nowhere quite lost. There was a chronic abscess below the left clavicle, otherwise his health was good. Mr. Heuston opened and drained the abscess, which gradually healed. He remained in hospital without any change till February, 1891, when he grew weaker and duller, and developed some pyrexia for the first time. He gradually became comatose and died.

On *post-mortem* examination, we found the spinal canal of normal size throughout. Around the theca in the dorsal and lumbar regions was some soft, white, putty-like material. The body of the tenth dorsal vertebra was soft and carious, and there were masses of white tuberculous tissue in the ninth and eleventh vertebræ. The cheesy matter was in no way adherent to the dura mater; the outer surface of this membrane was smooth, and normal in every respect. Its inner surface, however, was much diseased. At the level of the third cervical vertebræ, the dura began to be covered with a thin, soft membrane; passing downwards this membrane became gradually thicker, until in the lower dorsal

region there was a quarter of an inch of firm white tissue attached to the inner surface of the dura mater on the front and sides of the cord. Posteriorly, this tissue or false membrane was not so thick. In the lumbar region it gradually thinned off, so that the lowest part of the dura was quite healthy. This tissue in the lower dorsal region was firmly adherent to the arachnoid and pia mater; elsewhere there were only some loose adhesions to the arachnoid. On microscopic examination of the thickened dura mater, I found the outer surface, and most of the tissue of the dura to be perfectly healthy. The tissue on its inner surface was composed of granulation tissue containing many giant cells; a good deal of it had become caseous. In the giant cells I found a few tubercle bacilli.

This tissue, when it was thickest—that is, at the level of eighth and ninth dorsal vertebræ—had compressed the cord to about half its normal size, but its cylindrical shape was preserved. Above and below this compressed region the cord, on microscopic examination, showed the ordinary ascending and descending degenerations. As well as this the pia and arachnoid are much thickened, and unusually adherent to the cord; from the pia a quantity of fibrous tissue extends inwards into the white matter of the cord, forming a zone of peripheral sclerosis. The anterior longitudinal fissure is closed up with dense fibrous tissue adherent to the cord on each side. The nerve roots are surrounded by a quantity of fibrous tissue. As well as this there is a slight diffuse sclerosis throughout the whole of the cord, which affects all the white matter of the cord equally. The grey matter is normal. In the peripheral parts of the cord the blood vessels are extremely enlarged, especially in the lower dorsal region. The brain showed an acute tuberculous meningitis of the ordinary type. This was the immediate cause of his death. With this exception, the cephalic, thoracic, and abdominal viscera were perfectly normal.

Remarks.—The paraplegia, therefore, from which this patient suffered was caused by a growth of tissue, tuberculous in nature, on the inner surface of the dura mater. The question then arises, Where—in what tissue—did this tuberculous process primarily start? Caries of the spine, with secondary involvement of the dura mater, is infinitely the most common course for tuberculous disease to take. But there is a good deal to be said against this view—1. The

patient was eleven months in hospital—that is, until nine months after the paraplegia became complete—and during this time careful examination by my colleagues and myself could never detect any disease of the spine. 2. It was not until three years after the onset of paralysis that spinal deformity occurred, but, having once began, it developed very rapidly. 3. The appearances in the dura mater strongly oppose the idea that the disease extended from without inwards. In pachy-meningitis secondary to spinal caries, the external surface of the dura mater is inflamed, is rough with nodular thickenings, is covered with newly formed fibrous tissue, and becomes adherent to the bones, while the inner surface is normal. In my case it is exactly the reverse. The inner surface is affected, the outer normal. For all these reasons I have come to the conclusion that the primary lesion in this case was a tuberculous inflammation of the inner surface of the dura mater. Chronic primary tuberculous inflammation of the inner surface of the spinal dura mater is an exceedingly rare disease. Gower^a says—“In the very rare chronic tuberculous meningitis, the inner surface of the dura mater is covered with a layer or layers of tubercles in various stages of development mingled with inflammatory products. The arachnoid may also contain degenerating tubercles, and the two membranes may be united together by the new formation.” He gives one reference to a case described by Weiss, of Vienna. In Eulenberg’s “Real Encyclopædia,” Vol. XVII., p. 76, we read—“Weiss describes a case of pachy-meningitis interna tuberculosa. The dura was thickened; its inner surface was covered with a soft, greyish-yellow, granular layer. The arachnoid was tuberculous and was adherent to the dura.” I have made an extensive search to find further references to this disease, but I have not succeeded in discovering any.

^a Diseases of the Nervous System. 2nd Ed. Vol. I, p. 284.

HORNY EPITHELIAL TUMOUR.

BY ARTHUR H. BENSON, M.A. UNIV. DUBL., F.R.C.S.; AND
WILLIAM R. GRAVES, L.R.C.P., L.R.C.S.

[Read in the Section of Pathology, May 13, 1892.]

THE tumour, which was oval in shape and about 15 cm. long by 10 cm. wide, was removed, in St. Mark's Ophthalmic Hospital, from the outer side of the brow of a woman aged about thirty years.

It was situated not far from the left external canthus, and was believed (before removal) to be an ordinary dermoid, so common in that situation. It was easily enucleated, not being adherent to the bone or skin, and the wound healed as usual by first intention. I handed the tumour to Dr. Graves, who at once, on opening it saw it differed from any dermoid he had ever seen, and so hardened it and cut sections for microscopic examination. He will state the particulars of the microscopic examination, which the accompanying photograph [exhibited] will further illustrate.

Report by DR. GRAVES.—The tumour is completely enveloped in a firm fibrous capsule, which is joined by numerous trabeculæ from the interior. These trabeculæ pass between masses of horny epithelium; the latter contain, here and there, an island of Malpighian cells, from which these horny cells grow.

The tumour not being adherent to either the bone or skin could not have originated from either of them. Its origin becomes an object of extreme interest; the history pointed to a gradual growth from birth. As the tumour could not properly be described as a dermoid, I think that perhaps

“horny epithelial tumour” best describes it. Its position points to the possibility of its having originated in an enclosed group of unappropriated epithelial cells. The slowness of growth and excess of horny cells was possibly due to the very trivial blood supply which it received. Indeed, as far as I could make out, no blood-vessel entered the capsule, nor were there blood-vessels in the trabeculae.

Last year I showed in the Pathological Section a very similar tumour, removed by Mr. Wheeler from the neck of a boy. This tumour was macro- and microscopically identical in structure and history. (See Transactions of the Royal Academy of Medicine in Ireland, vol. IX., page 428.)

REPORT ON THE PRESENT ASPECT OF DENTAL CARIES.

By A. W. W. BAKER, M.D. ; F.R.C.S. ;
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[Read in the Section of Pathology May 13, 1892.]

IN writing this communication I have chiefly consulted Professor Miller's work on Micro-organisms of the Human Mouth, who, by his original researches in this department of pathology, has done so much towards elucidating its problems.

In the first place, I think it will be interesting to glance, in the briefest manner possible, at some of the theories which from time to time have been proposed, in order to explain this, one of the most widespread of diseases.

The following list of theories, ranging from the time of Galen to the present day, show the very different views which have been held :—

1. Depraved juices accumulated in the teeth.
2. Disturbances of nutrition.
3. Inflammation.
4. Worms.
5. Putrefaction.
6. Chemical dissolution.
7. Parasites.
8. Diverse causes.
9. Electrolytic decomposition.
10. Chemico-parasitic influences.

I will not attempt to criticise all the views here enumerated, but shall touch upon some of those which seem more important.

The inflammation theory seems to have had the support of Galen, and, to a certain extent, was favoured by John Hunter, who, having designated caries as mortification, said, "I am apt to suspect that during life there is some operation going on which produces a change in the diseased part."

In our own times a serious attempt to uphold this theory has been made by some American writers. However, without going fully into the subject, I may state that it is very generally believed that eburnitis, or inflammation of the dentine, does not occur, for the following reasons:—

1. None of the elements which appear in inflammation of other tissues of the body have been found in decayed dentine.

2. It is not possible to produce decay of a tooth by any of those mechanical or chemical irritants, which invariably produce inflammation in other tissues. And the operations for filling teeth, which are undertaken every day, show that it is possible to saw, cut, and file a tooth in all conceivable ways without any reaction as far as the dentine is concerned; such things, we know, are not possible in bone.

3. One of the most important reasons—that all the phenomena of caries may be observed in pulpless teeth, and in natural teeth, worn on plates, as artificial substitutes.

4. All the phenomena of caries may be produced artificially outside the mouth.

With regard to the putrefactive theory, it has been observed that an extracted tooth may be left almost an indefinite time without showing change in a putrefactive mixture; also, such a mixture generally gives an alkaline reaction, whereas an acid condition of the mixture is of primary importance in order to produce caries.

Up to recent times no theory of decay was more popular

than the chemical; yet, although cavities may doubtless be produced in teeth by the action of an acid, as has been shown by Magitot, still, the differences presented by a decalcified tooth and one the subject of caries are at once apparent under the microscope.

The Dutch scientist, Leeuwenhoek, was probably the first to observe the presence of micro-organisms in the mouth (1683), while Ficinus, a Dresden physician, attributed dental decay to their action, and proposed for them the term *Denticolæ*.

However, the first really important observations with regard to the parasitic theory of decay were made by Leber and Rottenstein, who regarded the commencement of caries as a purely chemical process; but as soon as the dentine is superficially decalcified, the elements of the fungus, *Leptothrix buccalis*, penetrate the dentine tubules, enlarge them, and thereby facilitate the rapid penetration of the acids. The communication by Messrs. Milles and Underwood to the Dental Section of the International Medical Congress of 1881, in London, marked an epoch in the history of caries. They demonstrated the constant presence of micro-organisms in sections of decayed dentine, and the widening of the tubules produced by them. They rejected the purely chemical theory of decay, and stated their conviction that in the decay of the hard tooth-structures two factors have been always in operation:—1. The action of acids. 2. The action of germs.

Since 1881, mainly owing to the researches of Prof. Miller, our ideas on the process of decay in teeth have assumed a very definite form. We believe that it is a chemical process in which micro-organisms are largely concerned. In other words, the decay of teeth is due to the solution of the lime salts by an acid developed in the mouth, and once the enamel ceases to be an impenetrable barrier, micrococci and bacteria

find their way into the dentinal tubes, flourish on the contents of the tubes, proliferate, and, by the production of liquefaction foci, break down the dentine in all directions, undermining the sounder portions of the enamel. The source of the acid, which is the primary cause of the mischief, naturally first attracts our attention. It is believed to be formed by the fermentation of the remains of food—principally the carbohydrates—in the mouth. The mouth being an incubator where all sorts of micro-organisms constantly flourish, the changes induced by them in the small portions of starchy foods and sugar which remain in and about the teeth, speedily develop acids, of which lactic acid is the chief. It is interesting to note that meat remaining in a cavity in a tooth does not become acid, but after the lapse of some hours gives an alkaline reaction. In order to demonstrate the above statements, I have made the following experiments, as suggested by Prof. Miller:—

1. In a sterilised test-tube I placed a mixture of bread, sugar, and saliva, and placed the tube in the brood-oven, at 37° C.; on being tested at the end of twenty-four hours it gave a strongly acid reaction, which was maintained for a week.

2. A tube similarly filled, before being placed in the incubator, was sterilised for half an hour in Koch's steam steriliser at 100° C. At the end of 24 hours it gave a neutral reaction, and was the same at the end of a week. This showed that the formation of the acid could be stopped by sterilisation. I then had further to determine how sterilisation of the bread and sugar alone and the saliva would affect the process; so in—

3. A sterilised tube I placed bread and sugar only, and kept this, as before, in the steamer at 100° C. for half an hour. Saliva was then added and the tube transferred to the incubator. At the end of the 24 hours there was a

well-marked acid reaction, which continued for a week. Sterilisation of the bread and sugar evidently did not effect the result; so in the fourth tube saliva alone was placed and sterilised in the steamer, then bread and sugar were added, and placed in the incubator. At the end of twenty-four hours this showed a very faint acid reaction, which it retained for a week. That this tube gave an acid reaction at all is, I think, due to the fact that bread nearly always has a slightly acid action; but there was a very marked difference between this tube and No. 3, in which the saliva had not been sterilised, in their effect on litmus paper. In stating that these several reactions continued for a week, I do not mean to convey more than that I tested these tubes daily for the first three days, then again at the end of the week, after which I discontinued the experiment.

From these experiments it would appear that we have to deal with an organised ferment in the saliva, which, acting on the carbo-hydrates, produces the acid.

It is not my intention to go into the question of the varieties of the micro-organisms to be met with in the mouth, as that, for many reasons, would require a separate communication, there being at present over 100 separate organisms which have been isolated from the mouth. All these do not of necessity occur in all mouths; however, there are six which are fairly constantly met with, viz. :—

Leptothrix innominata.

„ *buccalis maxima.*

Spirillum sputigenum.

B. buccalis maximus.

Iodococcus vaginatus.

Spirochæte dentium.

We see, then, that the first step in the process of caries is the development of an acid by the action of the mouth

micro-organisms on the carbo-hydrates clinging about the teeth, and that this acid, conjoined with defective tooth-structure, brings about solution of the lime salts. The subsequent stage being the entry of micro-organisms into the decalcified dentine, it will be interesting to note what are the appearances which a tooth presents under these circumstances.

Macroscopic Appearances of Caries.—The enamel first shows loss of polish on the surface, so that a fine point will not glide easily over it. Then a white, chalky-looking spot appears, with loss of translucency, the tissue gradually breaking down into a soft cheesy powder, and, as the enamel only contains 3 per cent. of organic material, it is unable to cohere once decalcification takes place, but is soon washed away in the secretions of the mouth. In chronic cases the enamel margins of the cavity thus formed become pigmented a dark brown colour. As the process spreads to the dentine we no longer find a soft, cheesy mass, but a tough, cartilaginous substance, due to the fact that dentine contains a much larger percentage of organic material, about 23 to 25 per cent.; consequently it can retain its form for a longer period. However, as the process of decay advances, liquefaction foci form, the dentine disintegrates, and is washed away in the fluids of the mouth. The destructive process spreading more rapidly in the dentine through the tubules and their branches accounts for the undermining of the enamel which is so frequently observed. Caries, however, spreads more rapidly along the tubules than through their finer and more tortuous branches, consequently the disease advances in a pyramidal fashion, the apex of the cone being towards the pulp cavity. This is specially the case in teeth of good structure, whereas, in poorly developed teeth, and those containing inter-globular spaces, the destructive process tends to spread more rapidly in a lateral direction.

In the cementum, owing to its bone-like structure, caries tends to the formation of wide, shallow cavities.

The cause of the transparent zone of dentine which is often seen surrounding the early stages of caries, which also may be observed in worn-down teeth, and in the roots of the teeth of old persons, is not quite a settled question, but is thought to be due to the consolidation of the dentine, so that it becomes more homogeneous. The various shades of colour which may be observed in a cavity in a tooth, are now believed to be due to chromogenic mouth bacteria. This pigmentation is chiefly to be observed in very chronic cases.

Under the microscope the changes which take place in the enamel are not very readily observed, owing to the enamel being washed away almost as fast as it is decalcified. Scrapings of this tissue reveal enamel prisms broken down and mixed with masses of micro-organisms. The dentine, however, affords beautiful preparations in which the process can be well studied. Here the chief characteristics are the enlargement and varicose condition of the tubules, the thickening of Neumann's sheath, the invasion of the tubules with micro-organisms, and the subsequent formation of liquefaction foci.

The enlargement of the tubules is caused partly by the loss of intertubular substance, and partly by the dislodgment of intervening tubules, all the tubules not being uniformly enlarged. By the enlargement of the adjacent tubules, and solution of intervening basis substance, which is possibly caused by the peptonising action of the micro-organisms, liquefaction foci are formed, which are generally crowded with micro-organisms. By the formation of these caverns, and the fusion of adjacent caverns, the dentine is broken up, becomes porous, and is gradually destroyed. The tubules may be often seen in groups crowded with micro-organisms, while other tubules lying between these groups appear free

from infection. We frequently find tubules invaded alone by cocci, others with rods, screw-forms, threads, or a mixed infection. It must not, however, from this be thought that we have special forms of decay due to these different varieties of micro-organisms, which may be distinguished one from the other. Up to the present no difference has been detected in the various specimens of caries; moreover, we meet frequently with tubules differently infected in the same specimen.

The thickening of Neumann's sheath, to which I have alluded, is best seen in cross sections, where the comparatively indestructible and thickened sheaths, standing out from the basis substance, produce the well-known pipe-stem appearance, the dentine looking as if built of clay pipe-stems. The enlargement of the tubules, by their mutual pressure, sometimes proceeds to such an extent as to produce five or six-sided figures.

All the appearances which I have described as occurring in caries under natural conditions, I have been able to produce artificially by placing sound teeth in a flask containing a mixture of bread, sugar, meat and saliva, the flask being kept in a brood-oven at 37° C.^a Some of the teeth I cut in slices, others I covered with wax, except at one point, so as to produce cavities similar to what we meet in the mouth. At the end of a few days decalcification had so far advanced that I could penetrate the specimens with a needle, and at the end of fourteen days I was able to cut sections from the slices of teeth. The mixture in which the teeth were placed showed a strongly acid reaction by the end of the first day; it was occasionally renewed, and never at any time became putrid. Hanging drop preparations of the fluid in the flask showed various micro-organisms, many of which were

^a This experiment was first successfully carried out by Prof. Miller, and has subsequently been repeated by other observers.

motile. On the addition of iodine and iodide of potassium solution, some of the micro-organisms stained blue, but it did not interfere with their movements in the least. The only difference that I was able to detect between the sections of natural and artificial caries, was that the micro-organisms in the latter parted more readily with gentian violet stain when washed in alcohol.

The existence in these specimens of large decalcified portions of dentine into which micro-organisms had not yet penetrated, seems to support Professor Miller's view as to the existence in natural caries of softened, but non-infected areas of dentine.

PERFORATING ULCER OF THE STOMACH IN A MALE AGED SIXTY.

By JOSEPH REDMOND, M.D., F.R.C.P.;

Physician to the Mater Misericordiæ Hospital ;
Pathologist to the Coombe Hospital, &c.

[Read in the Section of Pathology, May 13, 1892.]

THE specimen which I bring under the notice of the Pathological Section this evening is perhaps sufficiently uncommon to be worthy of being placed on record. Most physicians have, unfortunately, met with perforating ulcer of the stomach in females. In males, however, according to statistics, gastric ulcer is not of infrequent occurrence, as they suffer from this affection compared with females in the ratio of two to three, yet cases terminating fatally by perforation are not commonly observed. The liability to perforation of the stomach from ulcer in the female is greatest between fourteen and thirty years of age. In the male perforation of the stomach does not seem to occur more frequently at one age than at another, though the largest number of ulcers occurs between the thirtieth and fortieth year.

CASE.—The patient, J. C., aged sixty, a tobacconist by occupation, was admitted to the Mater Misericordiæ Hospital on the 26th March, 1892, complaining of nausea and vomiting, the ejecta being “coffee ground” in appearance. He stated that he enjoyed good health till about a year ago, when he began to feel pain after taking food. From time to time since then he had frequent attacks of dyspepsia.

State on admission.—The patient was pale, weak, and emaciated; the face thin, anxious, and cachectic looking. He vomited occasionally, complained of slight pain to the left of the epigastrium, and said that his bowels had been confined for three or four days.

He improved considerably under treatment, vomiting and pain having completely disappeared. On the 4th of April, at 6 30 p.m., nine days after admission, shortly after taking a cup of milk he complained of severe pain in the epigastrium. Soon his body became covered with a cold perspiration, temperature sank to 97°, and the pulse at the wrist was almost imperceptible. The patient now lay on his back with the legs drawn up on the abdomen, and complained of extreme abdominal tenderness, the slightest pressure causing intense pain. The abdomen was distended and tympanitic all over. He vomited frequently, the matter being coffee ground. After some time the patient rallied, but at 10 30 p.m. he again complained of severe pain accompanied by vomiting, and died in about twenty minutes in a state of extreme collapse.

The *post mortem* was made by Dr. M'Weeney twenty-four hours after death. The following are the notes:—

On opening the abdomen a quantity of foul-smelling gas escaped. The intestines and stomach were distended with gas, especially the latter. There was a slight coating of fibrinous exudation on the most prominent coils of intestine which could be scraped off in the form of a greenish semi-fluid matter; at the same time the vessels were markedly hyperæmic, so-called suction lines being absent. Pelvic cavity was full of slightly turbid yellowish fluid.

On examining the stomach, slight pressure caused a large quantity of coffee ground material and gas to escape from a small perforation in the anterior wall of the stomach. The opening was situated about one inch to the lower and mesial side of the œsophageal opening. The posterior wall of the stomach was found to contain two hard masses, the larger of which was intimately adherent to the pancreas. On lifting up the stomach the greater curvature was found to be adherent to the upper part of the mesentery by a number of tough adhesions, which included the mesial portion of the pancreas, leaving the head and tail of that organ free. The aperture in the stomach, viewed from the outside, was about the size of a sixpence and clean cut. On opening the stomach it was found to contain two large solutions of continuity, the larger of which was towards the pyloric region on the lesser curvature, and measured 4 inches by $2\frac{1}{2}$ inches. The long axis of the ulcer was not quite parallel to the lesser curvature but inclined to it at an acute angle; its walls were perfectly clean cut and more

than half an inch high. The floor of the ulcer at its posterior part showed several round elevations, which upon section proved to be part of the pancreas, of which viscus the floor of the ulcer in this region was exclusively composed. The smaller ulcer was towards the œsophageal end, and separated from the larger by a distance of about $1\frac{1}{4}$ inch; it was also on the lesser curvature. Its shape was nearly circular, and was about 1 inch in diameter. Its floor was irregular without any trace of hæmorrhage, and at one corner was the perforation which had occasioned the fatal termination of the case.

The specimen was preserved in Dr. Thoma's new saline preserving fluid, to which my attention was drawn by Dr. M'Weeney, and which I may say has fully justified the high account given of its preserving powers by its inventor.

SECTION OF STATE MEDICINE.

THE CONTROL OF INEBRIATES.

By E. MACDOWEL COSGRAVE, M.D., F.R.C.P.;

President of the Section of State Medicine in the Royal Academy of Medicine
in Ireland;

Professor of Biology, Royal College of Surgeons.

[Read in the Section of State Medicine, February 19, 1892.]

THERE are few things more difficult than the successful treatment of inebriates, not from advanced pathological change, but from want of power to keep them from the cause of their diseased condition. For cure, abstinence for a prolonged period is requisite, but, unless the patient is anxious to be cured, and capable of a strong effort, failure is almost certain to result, so many are the temptations, so easily is drink obtained. To put the patient out of the way of temptation is, indeed, rendered possible by Acts of Parliament, but the procedure is so difficult and the treatment so expensive as to be of but little use.

Medical men are far in advance of lawyers in recognising inebriety as a disease, and it was the interference of the lawyers in the House of Commons which kept the Habitual Drunkards Act from being more useful and workable.

The law has, however, for long recognised to some extent the disease element in inebriety. True, Sir Edward Coke, the highest legal authority of his day, wrote:^a—“A drunkard

^a Coke. Institutes. 247. Blackstone's Commentaries. Bk. IV., ch. 2.

who is *voluntarius dæmon*^a hath no privilege thereby; but what hurt or ill soever he doeth, his drunkenness doth aggravate it." But Sir Mathew Hale (middle of the seventeenth century) wrote:—"Although the simplex frenzy occasioned immediately by drunkenness excuse not in criminals, yet if by one or more such practices an habitual or fixed frenzy be caused, though this madness was contracted by the vice and will of the party, yet this habitual and fixed frenzy thereby caused puts the man in the same condition in relation to crimes as if the same were contracted involuntarily at first"—thus recognising the important distinction between mere drunkenness and that more advanced stage when the habit has turned into disease.^b

Perhaps the best description of the inebriate, as distinguished from the ordinary drinker, is that given by Dr. D. G. Dodge before the Select Committee on Habitual

^a "The laws claim that the drunkard is a voluntary madman. This is incorrect. The inebriate does not voluntarily renounce control; on the contrary, he believes he can control his actions, while he cannot. Though he does not know it, alcohol paralyses inhibition."—Dr. T. L. Wright, Bellefontaine, Ohio. Society for the Study of Inebriety. Jan. 6th, 1891.

^b It will be seen from the following extracts that the law on this point is still unsettled:—

"Leeds Assizes, December 11, 1891, before Mr. Justice Wright, John Routledge was indicted for causing grievous bodily harm with a red hot poker to Elijah Holmes, at Doncaster, on July 18th. His counsel, Mr. B. G. Wilkinson, urged that it was done while the prisoner was drunk, and unable to tell what he was doing. His lordship said it was impossible to accept the statement that the prisoner was drunk as an excuse in law. For doing a thing of this kind a man must answer whether drunk or sober. Society could not exist if such a plea were to be allowed. There was not, however, evidence showing that the prisoner was in a completely helpless state of intoxication."

"On the same day, before Mr. Justice Lawrance, Liverpool, John Miller was tried for the murder, under shocking circumstances, of his wife, Jane Miller, on the 13th November. Miller, who was a blacksmith, was a heavy drinker, and under the influence of drink when he battered his wife's head in with a hatchet, without any apparent cause. His lordship, in summing up, said: 'It was quite right that a jury should consider whether a man could drink himself into such a condition that he was incapable of forming any intention, and that he had no control over himself. If that was proved to them satisfactorily, then the crime would be reduced from murder to that of manslaughter.'"

drunkards (1872):—"We make a great distinction between the two classes. One man when he has taken one drink loses the power to control himself in that direction; another man can take several, and be able to stop. But we cannot for the life of us tell why it is that because a man has taken one drink he must go on until he is saturated with it, without any power to stop. We look upon that man as actually diseased with alcohol, and only that class.^a

The Inebriates Acts, imperfect as they are, have, at any rate, formally recognised the disease element in inebriates, who are thus described:—"Those who, notwithstanding the plainest considerations of health, interest, and duty, are given over to habits of intemperance so as to render them unable to control themselves, and incapable of managing their own affairs,^b or such as to render them in any way dangerous to themselves or others."^{c d}

^a Answer 3072.

^b A commission of lunacy was supported against a person who, when sober, was a very sensible man, but, being in an almost constant state of intoxication, he was considered incapable of managing his property.—Collinson on Lunacy. Vol. I., p. 71. Quoted by Dr. R. B. Grindred in *Bacchus*. P. 505.

^c "LAW OF PARENT AND CHILD IN JERSEY.—The Attorney-General appeared before the Royal Court, on Saturday, and called on the judges to deprive Mr. Nicholas Anthoine, clerk to the Impost Office, of the right of control or management of his children, he being an habitual drunkard, and that the said Court should appoint fit guardians for the said children. The Attorney-General stated that the persons directed to inquire into Mr. Anthoine's conduct had reported that he had often been seen drunk, and, whilst in that state, had danced in the streets, gathering a crowd around him, and was, consequently, unfit to be an example to a growing family, and unfit also to be entrusted with its control. The Solicitor-General, on behalf of Mr. Anthoine, contended that the articles exhibited were insufficient to warrant the Court in inflicting so serious a penalty on any man as depriving him of the control of his own family, and instanced his being able to conduct the affairs of his office as a reason against granting the prayer of the citation. The Attorney-General replied, again urging the prayer of the memorial. The chief and other judges confirmed the Attorney-General's demand, and ordered that the defendant's family be given into the guardianship of a proper person chosen by their nearest relations, with the approbation of the Court."—Quoted from a Jersey paper of 1837 in *Bacchus*. P. 506.

^d Report. P. v.

For practical purposes, the inebriates for whose treatment we require extended powers may be divided into two classes:—

I. Those who cannot resist the temptation to drink, but will at any cost or sacrifice gratify the craving.

We all meet such cases too frequently, and know the difficulty of influencing them. I have frequently explained that to go on drinking meant death, and have convinced the patients of the fact without at all altering their habits. I have stood by whilst patients were told that they were getting into financial difficulties and that nothing but a sudden pull up could avoid a crash; they have acknowledged and deplored the fact, but made no change. I have pointed out to fathers how they were destroying the happiness of their wives and children; I have pointed out to mothers the neglected condition of their children and the dirt of their homes. Both fathers and mothers have heard me, assented, mourned the fact—and continued drinking. Indeed, in many cases so hopeless have I felt that I have longed for a complete physical break down, such as an attack of *delirium tremens*, in the hope that the utter physical prostration might give a chance of cutting off the supply of stimulants, and that possibly after such a shock some impression might be made on the weakened moral nature, so that the habit thus broken might not be resumed.

Such cases as these are sometimes hereditary, sometimes acquired.

II. Those who suffer from intermittent attacks, from *mania apotu*, who go on for long periods, even months, with apparently no desire for drink, and then having tasted drink, suffered mental trouble, sometimes, with no discoverable cause, break out and drink, morning, noon, and night for days, perhaps even for a week. In one case I have noted, the attacks used to come about twice a year. The gentleman would lock himself into a room with a supply of brandy, no

one daring to interfere with him until at the end of two or three days all noise ceased; his relatives would then break open the door and find him comatose. Between the attacks he attended to a large business, and took a leading part in religious and charitable work. The description of such a case given by Besant in "The Demoniac" is scarcely an exaggeration of the reality.

The cases are generally examples of hereditary neuroses.

In both classes of cases "total abstinence is the only safe and sure road to recovery."^a

In the first class nothing else will stop the craving. As long as alcohol in any form is taken the wish for it will continue, but when it is given up for a time the craving becomes less. There is also improvement in the physical and moral condition, food can be better taken, the strength improves, and the patient's promise can to some extent be relied on.

In the second class of cases the natural tendency is for the attacks to become more frequent, and their effects more lasting, so that gradually the system breaks down. The only hope in such cases is to reverse this process, to tide over attacks, and so prolong the intervals and render the impulse more easy to resist.

To keep either class in their own houses from drink is more than difficult—their ingenuity, their unscrupulousness, and the certainty that some about them will help to supply drink, render treatment nearly hopeless. Not long ago, whilst helping to place a patient suffering from alcoholic paralysis

^a Dr. D. S. Dodge, Physician to the New York State Inebriate Asylum, before the Select Committee on Habitual Drunkards. In Answer 2973 he says:—"It is a physiological fact that when a habit has been formed or contracted, an absolute removal of the agent that produced the habit is necessary in order that a new state of things may become possible. Place before a patient a liberal supply of tempting and nutritious food, and ask him to starve himself to death; nature is stronger than the will—it is an impossibility. Place alcohol before him and ask an inebriate to cure himself gradually; it is equally an impossibility."

on a water-bed, I found a bottle containing whisky under her pillow, and yet for days she had been unable to leave her bed, and everyone who had access to her room professed to be trying to keep drink from her. In another case, which also ended fatally, nine brandy bottles were hidden about the bed. Even in hospitals we all know how difficult it is to prevent stimulants reaching our cases.

For successful treatment such cases must be admitted into institutions where they can be controlled—not allowed to have drink sent in, not allowed to go out for it. Our power in this direction is certainly limited. Patients must either go voluntarily into a lunatic asylum,^a where they cannot be detained if they wish to leave; or be sent to Belgium^b—a course which has manifest disadvantages; or be treated under the Inebriates Acts, 1879 and 1888, which, unfortunately, impose so many difficulties that they are made but little use of in England, and are altogether unused in Ireland. During 1890 only 109 patients were admitted into Retreats under the Acts,^c and this was a higher number than in any previous year.^d

The powers given by the Inebriates Acts (the expression “Habitual Drunkards” was struck out by the amending Act of 1888, which also made the former ten-year Act permanent) are briefly as follows:—

Justices of the Peace may grant a license for any period not exceeding thirteen months to any person or persons to keep a retreat for habitual drunkards. One of the licensees must reside on the premises and be responsible for its management, and if the licensee is not on the Medical Register a duly qualified medical man must be employed as attendant.

^a Under 29 & 30 Vict., cap. 57.

^b Cf. *British Medical Journal*. November 7th and 14th, 1891.

^c Eleventh Annual Report of the Inspector of Retreats under the Inebriates Acts, 1879 and 1888, for the Year 1890.

^d 1885, 77. 1886, 73. 1887, 66. 1888, 99. 1889, 78. 1890, 109.

No such license can be granted to any person licensed to keep a lunatic asylum.

An habitual drunkard who wishes to be admitted into a Retreat must apply in writing to the licensee, and state the time during which he undertakes to remain. The application must be accompanied by the statutory declaration of two persons that the applicant is an habitual drunkard, and his signature must be attested by any two Justices, who must state that he understood the meaning of his application, which must have been explained to him. The applicant may then be detained for the whole of the specified term, which must not exceed twelve months.

From the passing of the first Act medical men have constantly complained of its inadequacy, and the very small improvement made by the amending Act has been deplored. The subject of the deficiencies of the first Act has already been brought before this Section by Dr. Tweedy.^a The frequent discussions of the subject by the Society for the Study of Inebriety has educated public opinion in the same direction, and has helped to formulate our requirements. The special committee of the British Medical Association has also done good work.^b

In Scotland feeling is so strong that, in 1890, Mr. Morton's excellent Restorative Homes Bill, approved by leading members of the medical and legal professions, was introduced into Parliament. A similar movement is going on in other

^a State Control of Chronic Inebriates. Transactions of the Academy of Medicine in Ireland. 1884.

^b "For years past your committee has, with the Society for the Study of Inebriety, been urging on the Government the need for an inquiry into the working of the Inebriates Act, 1879."

"Existing legislation has these chief defects: There is no provision for the poor; admission of a voluntary applicant for reception in a licensed retreat is rendered forbidding by an enforced appearance before two Justices; and an inebriate can be received and detained in such a retreat only at his own request."—Extracts from Report of Inebriate Legislation Committee to the British Medical Association. 1891.

countries—even in America, where legislation on this matter is much in advance of England. An important bill, in which the Emperor is personally interested, is likely soon to become law in Germany.

Now to deal shortly with what is required.

I. Voluntary entrance into an inebriates' home should be made much easier. The necessity of appearing before two Justices is especially odious. It is not at all easy to get two Justices, and the [consequent delays and disappointments (added to the dreaded publicity) have kept many cases from treatment. Surely one Justice ought to be enough. The British Medical Association and the Society for the Study of Inebriety have urged this change, which has also been recommended by the Government Inspector of Retreats.^a As one magistrate can at present commit a person of unsound mind to a lunatic asylum, surely one might be trusted to commit an inebriate to a retreat.^b

It would, indeed, make the Act much more workable if entrance was still more simplified, and no appearance before a magistrate required. In Mr. Morton's Restorative Homes Bill it was suggested that in entering a Home the transaction should be merely with the superintendent of the Home. The application (signed by the applicant and two respectable witnesses) engaged that he would remain in the Home subject to the provisions, rules, and regulations until discharged. One of the provisions was that the time of detention should be twelve months, unless circumstances rendered it expedient to discharge him sooner.

Dalrymple's original Bill had a practically similar provision. With all the safeguards for inspection and appeal that are in the present Acts, no harm could arise from entrance into a Retreat being made comparatively easy.

^a Seventh Annual Report. 1886.

^b A declaration before one Justice is required in South Australia, Victoria, and New Zealand.—Inebriety. Dr. Norman Kerr. Ed. 2, p. 378.

II. There should be power to send people to inebriate asylums; this power to be invoked by the person's family or friends, or by the public authorities. Magistrates to have power to commit, or this power to be given to an inspector, who should summon a small jury to try the case, the accused to be present and heard in his own defence. The very possibility of such action being taken would have a restraining effect in many cases. If able to pay there should be power to compel payment.

III. Provision for poor cases is urgently needed. This would, of course, cost money, but if it led to the cure of inebriates it would lessen police court, workhouse, prison, and lunatic asylum^a charges. The cost need not fall on the general body of the ratepayers. The inmates should be required to work to help to meet the cost of their support, and part, if not all, of the residue might fairly be drawn from the trade which makes a profit out of inebriates. This would be no harder on "the trade" than Mr. Ritchie's proposal that they should form a fund to buy up their own, so-called, vested interests. This plan has been adopted in other countries. The Inebriates' Home of King's County, New York, gets 12 per cent. of the license moneys.^b This is an example which might well be followed.

The small fines and short terms of imprisonment at present imposed for drunkenness are wholly inadequate. The Act regulating the fines is 4 Jac. I., c. 5, and prescribes a "forfeiture of 5s., or the sitting six hours in the stocks, by which time the statute presumes the offender will have regained his senses, and not be able to do mischief to his neighbours."^c

^a "I am greatly concerned that, whatever principle is adopted in future legislation in regard to the restraint of drinking, the 400 cases who year by year in Scotland are made absolutely insane by drink shall in some way be considered and provided for, as well as the ordinary habitual drunkards."—Seventy-seventh Annual Report of the Royal Edinburgh Asylum for Insane.

^b Inebriety. Dr. Norman Kerr. Ed. 2, p. 358.

^c Blackstone. Bk. IV., ch. 4, x.

That was very good treatment when drunkenness was only occasional, but for the chronic "drunks" of the police courts seven days' imprisonment just gives them time to pull themselves together at the expense of the ratepayers, and to be ready for a fresh spree when they are discharged. It would be much better that the sentences should each time be made longer, and that anyone who was convicted of drunkenness a certain number of times ^a in one year should be committed to an inebriate asylum.

In New York the trustees of the Inebriates' Home can visit the county jail and chose from the imprisoned for intoxication or habitual drunkenness such as they think fit subjects for the Inebriates' Home. On the certificate of the President of the Home the jailer hands over the prisoner to the authorities of the Home.^b

IV. There are defects in the present Acts which unnecessarily add to the difficulties of the licensee.^c Thus a patient can easily escape if he likes, and the licensee has no right to use force to prevent him; and so he can go out, get drunk, and come in and make a disturbance. There is no punishment for such a patient except for having had drink, and the Home Secretary will not prosecute. When expenses are incurred by the issue of a warrant and by bringing the patient back from a distance, there is no power of compelling the patient or his representatives to pay them. There is no punishment for a patient who asks for an inquiry if his complaint proves to be frivolous and vexatious.

V. The part of the liquor traffic which consists in encouragement of drunkenness should be suppressed.^d I have

^a In South Australia any person convicted of drunkenness three times within six months can be committed to a Retreat.

^b Inebriety. Dr. Norman Kerr. Ed. 2, p. 358.

^c Cf. Dr. F. J. Gray's paper, read before the Society for Study of Inebriety Oct. 3rd, 1888.

^d There is a statute in Pennsylvania which provides that saloon-keepers shall be held responsible in damages for injuries resulting from their sale of liquors

known a husband caution the publicans and spirit grocers of the locality not to supply drink to his wife without his word having the slightest effect. His things were pawned, drink was procured, and in some cases the bills for it were sent in to him, the dealer calculating that he would pay rather than make public his misfortune.

It has been suggested that a "Register of Drunkards" be kept by the police, and copies given to dealers in liquor, who shall not be allowed to sell to any on the list.

In Prince Edward's Island when proper notice in writing is given to any dealer in intoxicating liquor that a person is addicted to drink, he is not allowed to sell him any intoxicant. The penalty for the first offence is a fine of not less than £5; for subsequent offences a fine of between £5 and £10 and imprisonment for a period not exceeding thirty days.^a

I have brought this subject before the Section, believing that in many cases inebriety is a disease closely allied to insanity and susceptible of successful treatment, if power is given to keep the patient from drink for a sufficiently long period; and believing that the sooner the case is taken in hand the more is the probability of cure. I trust that my paper, short and imperfect as it is, may be the starting point of an important discussion.

to intoxicated persons. The Supreme Court of the State has lately made a decision which sustains the law as constitutional and equitable. A poor widow sued a liquor seller because he sold liquor to her husband until he was unable to guide his steps homeward, fell into a gutter, contracted pneumonia, and died. A jury gave the widow substantial damages, and upon appeal the Supreme Court sustained the verdict and the law. It brushed away without much ceremony the pleas made for the saloon-keeper that pneumonia, and not liquor, was the immediate cause of death, and that the man took the liquor voluntarily. The Court thus replied to the last plea: "Every drunkard not only takes liquor voluntarily, but whenever he can get it, and because of his weakness the law makes the saloon-keeper responsible for selling to such persons. He has not the will-power to resist the temptation, and for this reason the sale to him is forbidden."

^a Inebriety. Dr. Norman Kerr. Ed. 2, p. 367.

DR. DAVYS considered Dr. Cosgrave's paper a valuable contribution to the treatment of the inebriate. With reference to the second class referred to by Dr. Cosgrave, the intermittent drunkard, Dr. Davys considered (and quoted cases in support of his assertion) that the only successful treatment or cure of the intermittent drinker is, the moment his family call on their physician to prescribe for the inebriate, for the doctor, with the approval of the family of the patient, to employ a strong male-attendant to wait on the inebriate, and by physical force prevent him taking any alcohol, the patient to be kept in his house. Much greater privacy is thus observed than by sending him to a Retreat; and having to bring him before a magistrate, as suggested in Dr. Cosgrave's paper, is obviously objectionable. The adoption of the course suggested by Dr. Davys effects a cure so far in about three days as to enable the inebriate to resume, in perfect sobriety, his ordinary avocation. The same course should be adopted when, at the succeeding period, sometimes in two or three months, the patient breaks out again. The latter, when returning to his sober state, fully approves of the course adopted by his physician.

MR. EDGAR FLINN thanked Dr. Cosgrave for his most interesting paper. He quite agreed that the regulations for getting dipsomaniacs into Retreats under the Habitual Drunkards' Acts were made too stringent. They should be more relaxed, and such places would be much more availed of. It was a pity that the Habitual Drunkards' Act was not applied to Ireland, for it certainly was required, and would act most beneficially. Retreats in England for habitual drunkards did good in some instances, but in Mr. Flinn's experience they always failed in a cure, unless the patient was committed to the Retreat under a warrant. Persons addicted to drink were a class very difficult to treat by drugs; the radical cure, no doubt, was to remove them to some place where they would be prevented from obtaining drink—in fact, in some instances, they might with propriety be placed in asylums.

DR. J. W. MOORE instanced three cases of intermittent inebriety which had come under his observation, and expressed the opinion that physical restraint was a much more reliable means of treatment than any medicinal remedies. He alluded to the unscrupulous way in which vendors of alcoholic beverages supplied the unhappy victims of inebriety with wine or spirits. He considered that the use of the terms "beastly" and "bestial," in connection with drunkenness, was a libel on the brute creation. The whole ques-

tion of the control of inebriates was well worthy of the attention of the legislature of the country.

DR. THOMAS DONNELLY agreed with the previous speakers as to the value of physical restraint in the treatment of inebriates, and the necessity which exists for greater power in using it.

DR. COSGRAVE, in reply, said that intermittent inebriety was undoubtedly the most interesting form from a scientific point of view. Dr. Davy's treatment of restraint by a stalwart attendant was good, if the patient would submit to it; but there was no power to compel him to do so. Easy admittance into an inebriate asylum for a short period exactly meets such cases, affording a chance of tiding over attacks. Inebriety often caused neuroses in the descendants, sometimes whole families being affected with hysteria, epilepsy, inebriety, &c. In one case he (Dr. Cosgrave) had seen inebriety once in three generations. A lady, aged nearly seventy, suffered from intermittent attacks, her daughter died of *delirium tremens* at about thirty, and her granddaughter was seen under the influence of drink when only fifteen years old. The Reports of the Somerset House authorities show that alcoholic drinks are now seldom adulterated, so the evil done must be laid to the charge of the alcohol itself.

THE POLLUTION OF THE SOUTH-EASTERN FORESHORE OF DUBLIN BAY.^a

By D. EDGAR FLINN, D.P.H., F.R.C.S., M.R.C.P.;

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[Read in the Section of State Medicine, February 19, 1892.]

It is an admitted and uncontroverted fact plainly visible that the foreshore extending from Merrion to the Dalkey end of Sandycove is polluted and infested with the sewage from the adjoining districts of Booterstown, Williamstown, Blackrock, Seapoint, Monkstown, Kingstown, Glasthule, and Sandycove. This pollution has existed now for some years, and each sanitary authority has in turn made some slight endeavour (one might say a very minimum endeavour) to remedy the existing state of affairs; nothing in the way of a radical cure has been effected, and the insanitary condition that obtains along this stretch of coast line is nothing short of a gigantic public nuisance, and is anything but creditable to those who are vested with the powers to remedy it.

Procrastination and peddling have been the order of the day on the score of economy, and this, mark, at the risk of endangering the health of a large, important, and increasing population. Truly the methods in which the most vital

^a Since this paper was communicated to the State Medicine Section the position of affairs has somewhat altered in regard to the main drainage question; it now seems probable that the sanitary authorities of both Kingstown and Blackrock will promote separate schemes in the coming session of Parliament (the latter township has already moved in the matter). The Blackrock Board failed in the carriage of their Bill through Parliament last session.

clauses of the Public Health Acts are suffered to be administered in Ireland are beyond comprehension. It seems to be an accepted maxim that "What is, is best, and thus let it remain." Possibly when Macaulay's New Zealander will be in the flesh we may hope that the administration of our sanitary laws will then have reached the climax of perfection; but to this present generation it is all important that their lives should be spent amongst healthful surroundings, and for health's sake, without consideration of commercial value, sewage matter must be got rid of at any cost.

The residents along the foreshore in question have now for a long time been bitterly complaining of the pestilential odours that exist, and have at intervals protested against its continuance. The efficient drainage of any district is all important, but more particularly is it vitally essential in towns claiming to be health resorts, and desiring to be recognised as such. At certain seasons of the year, particularly in the autumn, the odour of sewage is strongly and markedly perceptible, and anyone who has walked along the coast-line from Blackrock to Kingstown when there is low water-mark will have, no doubt, noted this. It could not be otherwise, for at every few hundred yards there are drains carrying the sewerage matter from the adjoining terraces into the Bay.

At Salthill, near the West Pier, there is an outfall sewer which is nothing more or less than a public nuisance. The sewage of Booterstown and Williamstown is also discharged into the Bay. Blackrock, Kingstown, and Sandycove are also offenders in the same manner, and suffer the sewage to be discharged at several points along the foreshore. Now, if these several outlets were abolished, and one main system of drainage for the whole area adopted, the several townships would be materially benefited and improved. Sooner or later a main outfall must be sought for; it is absolutely

impossible that this wholesale pollution of the foreshore can be tolerated, and allowed to exist much longer. Where that outfall should be is the vexed question, but it must be conceded that some system other than that existing is urgently needed. This constant eyesore and menace to the health of the inhabitants must be grappled with, and the time is now opportune for something to be done.

The Blackrock Commissioners some time since applied to Parliament for a Bill to carry out a main drainage scheme from Merrion to Sandycove. The Kingstown sanitary authorities were not pleased that the smaller township should thus peremptorily seek to obtain an Act of Parliament to enable them to execute this main drainage scheme, and a determined opposition to the project was instituted in consequence, although it is on record that the latter Board quite recently approved of the same scheme, but yet lacked the enterprise to undertake its execution. It is to be regretted that two such important public bodies should be placed in antagonism on a question that each individual member acknowledges is one both of urgency and of necessity.

Public bodies should be above the petty disputes that are incidental to private life, and when the health of nearly 30,000 people is concerned, it is lamentable to see those who are elected to be the guardians of the public health procrastinating, delaying, and obstructing measures of sanitary reform that are urgently needed. This main drainage question is not a new one, it has been a stalking-horse before the public for the last twenty years or more, and is another proof of how slow are the methods and ways of sanitary administration. If a similar condition existed on such an important foreshore in Great Britain the necessary remedial measures would have been undertaken and executed long since; moreover, let it be understood that this main drainage scheme promoted by the Blackrock Board

is identically the same as that favoured by the Kingstown Commissioners in 1889, for which they invited and received tenders. Now they strenuously oppose the very scheme that they themselves were originally the sponsors of, and which, if carried out, would not only enhance and improve their own township, but would be a lasting public benefit to the health of the inhabitants residing along this stretch of foreshore. It has received the sanction of the Local Government Board, the late Chief Secretary, the Port and Docks Board, and the War Office. This main drainage scheme provides for deep sea outfall at a point north of Bullock—that is, the sewage in its crude state will be discharged into the sea at that point. Now, as we are aware, there are various methods for the disposal of sewage, and each of them have has its advocates; there are the several methods of sewage treatment—by precipitation, simple filtration, intermittent downward filtration, irrigation. In the first process, that of precipitation, the main object in view is the purification of the sewage by the introduction of chemical agents. The system of intermittent filtration has been very favourably reported upon by the Rivers Pollution Commissioners, and the process of irrigation of land or sewage farms has found favour in many parts of England, and where suitable, it has fully met all the requirements attaching to the disposal of sewage, but it is an admitted fact that no one system for the disposal of sewage, could be adopted for universal use. But for health's sake, without consideration of commercial profit, sewage matter must be got rid of at any cost, and what will best suit the requirements of an inland town or district will not be applicable to sea-bordering towns.

For the disposal of the sewage from the townships under consideration various methods have been suggested other than the scheme promoted by the Blackrock sanitary authority—reclamation of the strand of Merrion and Sandymount, and

the establishment of a sewage farm; some have advocated the Shone hydro-pneumatic system, which we hear is successful at Eastbourne and Southampton, with an outfall near Poolbeg.

The most recent system of sewage purification—viz., the “Oxygen Process,” patented by Messrs. Adeney & Parry, has also found many warm advocates. This process promises well, as we read that “One of the most interesting features of this process is the separation of the solid matters without the addition of precipitation chemicals, and the manner in which the air-drying of these matters, and the conversion of them into a form valuable for manurial purposes is effected without creating a nuisance in the neighbourhood of the works.” This absence of the addition of precipitating chemicals (in themselves a proved nuisance) is a great advantage, and is a step well in advance.

Another scheme by Messrs. Price and Going has been suggested—viz., a purification of the sewage by peat, which appears to have been very successful at Hitchin in Hertfordshire, a town with a population of 10,000. This system appears to have great advantages over the precipitation process by chemicals, as the resulting sludge is reputed to be inodorous, and the effluent inoffensive.

The deep sea outfall scheme, designed by Mr. Strype^a and accepted by the Blackrock Board, and some short time since by the Kingstown authority, encountered a considerable deal of opposition. It was embodied in a Bill presented to Parliament, and the effect of the Bill, as explained recently by the chairman of the Blackrock Commissioners, was to authorise the formation of a joint Drainage Board, composed of Kingstown and Blackrock Commissioners in certain proportions—the duty of the Board to be to cleanse and purify the foreshore of both townships from the point near

^a Mr. W. H. Radford, C.E., of Nottingham, has also lat-ly submitted a scheme, of main drainage, as has Mr. J. Berry, C.E., and Mr. Hart, C.E.

CHART OF DUBLIN BAY

SHOWING PRINCIPAL SEWAGE OUTFALLS.



W. KAYE PARRY, M.A.
CIVIL ENGINEER.

Boosterstown, where the Pembroke Township adjoins that of Blackrock, to the point near Bullock, where the Dalkey Township adjoins that of Kingstown. The work was to be accomplished by the construction of an intercepting sewer, into which all existing sewers, and any which might be made hereafter, should discharge, thus keeping the whole foreshore of both townships free from pollution. The cost of the work was estimated at £50,000. The intercepting sewer would be constructed, as far as Blackrock and the western part of Kingstown are concerned, along the unused rampart on the sea side of the railway, and in the eastern part of Kingstown between the harbour and the railway, till a point beyond the Victoria Baths was reached, where the sewer would be wider, and covered by a concrete roadway forming a beautiful esplanade for the use of Kingstown people and their visitors. This wide sewer would be sufficiently large to retain the sewage during the flow of the tide, and from this tank-sewer the outlet pipe, three feet in diameter, would be carried right under the sea to the point of discharge north of Bullock, near a place in the sea called the Leek Rock. This outlet was to be about 600 feet from the shore, and at a depth of 30 feet below the level of low water, and the discharge would only take place at ebb-tide. An automatic arrangement would effect this, and also prevent the flood-tide from entering the sewer. The tidal currents at this point have been very carefully observed, and there is no reason to suppose that any sewerage discharged there could possibly be carried back to the shore. On the contrary, the set of the tidal currents would carry all out to sea.

There appears to be great fear that portion of the sewage matter, at its discharge at the outlet, will be returned to the foreshore at Sandycove and Dalkey. When it is remembered that the discharge will only commence to take place at the first turn of the ebb-tide, and

at a depth of 30 feet below the level of low water, and at a point clear of any headlands, it seems most improbable, nay, impossible, that such a contingency should arise, and in confirmation of this, Dr. Carte (who has been a long time a resident in the district, and who is thoroughly conversant with the currents, especially in that part of the Bay) writes—“How, may I ask, can it be contended by the residents of Dalkey that the outfall for the sewage of Blackrock and Kingstown placed at Leek Rock—the position of which I am well acquainted with—can affect Dalkey, especially when the sewage is to be discharged, not like our own, at all tides, but at ebb-tide into deep flowing water, and at a point clear of our headlands? I claim to be as well acquainted with the tides and currents as anyone in the township. Furthermore, I accompanied the late Dr. Tufnell in his experiments as to the ultimate destination of the Dalkey sewage, Dr. Tufnell having been a witness amongst others who demonstrated to the Local Government Board officer that, owing to the course of the tide, the sewage of the township would be carried out to sea, I now take the liberty of saying that my own personal observation for many years confirms the fact that the ebb-tide current passes from the point of discharge of our sewage through Dalkey Sound, away to the north-east of Dalkey Island out to sea. And notwithstanding that the sewage of Dalkey Township is discharged at all stages of the tide, no trace of sewage remains on any portion of our coast-line with the exception I have mentioned, of the few places in the immediate vicinity of the outfall.”

A suggestion has been made that the West Pier main outfall should be carried to nearly the extremity of the Pier, to obviate the nuisance in that portion of the foreshore. This local alternative would be quite on a par with the do-little and do-nothing policy of the last few years. What is urgently required is a scheme embracing the whole area

under consideration. Engineers of the highest rank (Mr. Hassard and Major Isaacs, M.P.) are of opinion that two townships situated as are Kingstown and Blackrock, having natural deep-sea outfalls and strong tidal currents near, should avail of a deep-sea outlet. The neighbouring township of Dalkey has a deep-sea outlet, and we hear little about the foreshore being polluted for miles, in consequence of the sewage being discharged into the sea. Further on down the Bay we have the Township of Killiney and Ballybrack; as to how the sewage is disposed of in this picturesquely-situated township I am not aware, but I am led to believe that it is anything but satisfactory. If it were possible to have one main drainage scheme, embracing the four Townships of Blackrock, Kingstown, Dalkey, and Killiney, with a deep-sea outlet south of the latter township, a real and lasting work of sanitary reform would be accomplished, and a pure foreshore from Dublin to Bray would be the result. As a sequence of the foreshore pollution, the bathing places at the West Pier, Kingstown, Monkstown, and Blackrock, have long since been closed.

A similar deep-sea outfall system to that proposed for Kingstown and Blackrock exists at Llandudno, and its Medical Officer of Health is enabled to say that "there is probably no town in the kingdom, seaside or inland, where a better system of sewage obtains than here." The sewage is carried out into the sea at a point beyond low water mark; at the end of the main sewer pipes there is a tank-sewer over 1,000 yards in length, and 6 feet by 4 in diameter. In this capacious tank the sewage is stored during the time the sewer is tide-locked, but he states that no nuisance is created by the sewer-tank. What is known as the separate system of drainage is in use at Llandudno, that is, there are two sewers—one devoted to the conveyance of the sewage proper, and the other is utilised for the carriage of surface and sub-

soil water only. The sewage proper is carried into the sewer-tank undiluted with any surface water, and we hear that there is no nuisance created, or offensive smells in connection with this large volume of pure sewage matter being locked up for some hours in the sewer-tank. Fears are entertained by some that the tank-sewer proposed to be constructed in connection with this scheme will create pestilential odours, but Llandudno gives the answer, and let it be remembered that this scheme has been in operation since 1876.

At Portsmouth and Newhaven—the former with a population of 130,000 draining to the works, and the latter with a population of 4,000—the main drainage is carried out with sea outlets and with satisfactory results, and without causing a nuisance by fouling any portion of the foreshore. It may be in the recollection of many that a sad disaster occurred at Seapoint some two or three years ago, wherein a family were poisoned by eating shell-fish gathered near Seapoint. The inference then adduced was that the deaths were in great part attributable to these persons eating shell-fish which subsisted on sewage products. The place in question where these shell-fish were gathered is nothing more or less than a large cess-pool receiving the drainage of the terraces adjoining along the coast-line to Kingstown and Sandycove. The number of small drainage outfalls on to the foreshore are very many; the residents in the terraces along this line of foreshore are always complaining of the unpleasant odours at certain times.

At Booterstown there is one huge open drain, running along by the side of the line of railway, which, I am informed, receives a very large proportion of the sewage of the district. The stench emitted in this locality sometimes is quite as bad, if not worse, than that at the West Pier, Kingstown.

The Kingstown sanitary authorities have, no doubt, carried

out some improvements conducive to the public health; notably domestic scavenging and cleansing, a branch of urban sanitary work which has produced good results. On the question of main drainage they have been inclined to be apathetic, but possibly now that the smaller and neighbouring Township of Blackrock has made an advance-move on the question, they may take heart and set to work conjointly with it to find some remedy for the cleansing of the foreshore, which everyone admits is in a notoriously insanitary state, and which must eventually, sooner or later, have a marked effect on the health of the people.

Let us have some system of drainage, be it precipitation or the deep-sea outfall system. Presently the sanitary authority may have to cope with an epidemic and a large bill of mortality in consequence of the continued neglect of the districts controlled by them.

I have to offer my grateful thanks to Mr. Kaye Parry, C.E., who has been good enough to allow me to utilise his map showing the number of sewers emptying themselves on the coast.

DR. DONNELLY remarked that, as precipitation has been so successful in many places, it could be adopted at the Salthill outfall, and avoid polluting the sea so close to the best bathing-place near Dublin—that is, Sandycove.

DR. COSGRAVE strongly urged the importance of not carrying out new drainage into the sea, but of substituting precipitation as used so successfully at Drumcondra, and as about to be introduced in Dublin. At Llandudno the drainage is successful—for Llandudno, but not for the Conway estuary, which, fortunately, is not fringed by houses as Kingstown and Dalkey are. Messrs. Parry and Adeney's method seems the most promising.

DR. PARSONS asked if any bacteriological investigation of the effluent liquid after precipitation had been made, and if so with what result?

MR. FLINN replied.

ON PRIMARY SYPHILIS AND TUBERCULAR PHTHISIS.

By NINIAN M'INTIRE FALKINER, B.A., M.B., Univ. Dubl. ;

Diplomate in State Medicine, Trin. Coll. Dubl. ;

Fellow of the Royal College of Physicians, Ireland.

[Read in the Section of State Medicine, May 6, 1892.]

THE subject which I have the honour of laying before you for consideration is not original, and has been over and over again discussed in public; but as no remedial measure has resulted from the discussion, and the want of legislation is almost daily apparent to me in the discharge of my duties in Grand Canal-street Dispensary. I have determined to introduce the question of notification of the diseases "pulmonary tuberculosis" and "primary syphilis" briefly before you for discussion; and I would further suggest that, if the majority of the members of the Academy present here to-night agree with my view, we should send a recommendation forward to the Council of the Academy, asking them to forward a resolution to that effect to the Local Government Board of Ireland.

With reference first to "pulmonary tuberculosis," the following figures taken from the Registrar-General's reports for five years—

		Phthisis		All other Zymotics
		Ireland	Dublin	
1884	-	10,583	1,424	—
1885	-	10,709	1,426	—
1887	-	10,329	1,422	—
1888	-	9,812	1,274	8,164
1889	-	9,951	1,174	6,884

show that there is a yearly death-rate in Ireland for con-

sumption of about 10,000, of which deaths about 1,200 take place in Dublin, the average yearly death-rate for all other zymotics being about 7,500. These figures show an enormous death-rate for consumption, which, if it is a contagious disease—a view that I hold strongly to myself—we must conclude that the sanitary advisers of the Government of this country have been guilty of neglect in not having long since isolated the victims of the disease, and thus endeavoured to protect the health of the people.

I do not consider that it is the province of this paper to mention in detail my own *too*-frequent experience of these pitiable people, but I may mention that within the past week I have twice heard the deadly thud over a consolidated apex; once in a young man twenty years of age, whose father has just died of phthisis, and in whose room he lived; once in a young woman of thirty years with a family of three children.

To meet the requirements of these cases, and to protect the public health, I propose that there should be notification of this disease to the sanitary authority in whose district it occurs by the medical practitioner who sees the case. In addition, I recommend the appointment of a “public bacteriologist,” to whom the sputum should be sent for examination and report. So far it is easy to proceed, but a difficult question now arises as to removal or isolation of the patient. So long as the individual was able to work or take open air exercise I do not think it would be right to remove him, but it should be insisted that he should occupy a room to himself. But in the class of case that so often comes under my notice, when the victim has succumbed and is lying unable to work or rise from bed in a tenement room, with several other occupiers, I think the law ought to be promptly carried out.

If this were done, I believe that several hundred persons dying of consumption would be removed from the tenement

rooms of Dublin; and it is fair to argue, with the removal of the contagious material, that we might hope for a marked diminution of the disease and death that my friend, the Registrar-General, has the painful duty of making public.

In dealing with the question of legislation for the prevention of syphilis, those in favour of doing so have always met with great opposition, and that from a high-minded and religious section of society. This opposition has been so successful that, at the present date in her Majesty's dominions over which the sun never sets, British subjects are exposed by the misguided religious zeal of a certain class to one of the most dangerous and persistent forms of disease. To prove that, from a public health point of view, the present conditions of the law is unsatisfactory, I may mention that within the last two months the following cases of primary syphilis have come under my observation:—

1. A, age twenty-two, unmarried, envelope maker, living with her parents.
2. B, age eighteen, unmarried, living as a domestic servant.
3. C, age eighteen, living with her parents.
4. D, age twenty, unmarried, no occupation, living with her brothers and sisters.
5. E, age seventeen, unmarried, primary sore, rash, sore throat, suppurating bubo, housekeeping for her brother.

These cases, occurring in the course of a few months in girls who were not recognised as prostitutes, but living as virtuous members of society, with respectable people, discloses an alarming and insidious danger to the public health of the city. This feeling was riveted on me when I saw the last named of these girls performing her duties as a housekeeper and selecting meat in a butcher's shop—she was evidently hard to please, for she inspected and handled whatever came within her reach.

I may remind the Academy that some years ago the ques-

tion of the relations of the law to syphilis was a burning one, and that some of the leading members of our profession in Dublin were not afraid to plead for legislative interference to check the spread of the disease, even at the expense of a loss of patients, so strongly and wrongly do some people think on this question. So great was the outcry against the so-called "Legislation for Vice" that the Acts were suspended in all directions, even in the garrison and seaport towns of the empire. I hold that there should be legislation to check the propagation of syphilis and venereal disease, and that two sections should be legislated for—

1. The professional prostitute, who should be under systematic inspection and disinfection by the Public Health Office.

2. Those unfortunate persons who in a moment of weakness have become the victims of a human brute, and who lose not only their virtue but their physical health by his contact.

I would assume that all these persons were innocent, and deal with them kindly and cautiously, so as not to drive them by stringent legislation from the chance of a virtuous and healthy life, and recommend that there should be a ward in each of the Dublin hospitals set apart for the reception of "primary syphilis," in order to keep them apart from the society of the hopelessly irreclaimable inmates of a Lock hospital, and to bring them under the influence of a lady-superintendent, or somebody appointed by her, who would undertake the task of trying to regenerate a *fallen sister*.

The present "want of State control" is, to my mind, a blot on the government of this country. Few of us who have passed through the vicissitudes of a medical student's career but can appreciate the deadly perils which assail the young men of the city. At every corner he is subjected to the solicitations of a prostitute, who, I believe, is in three cases out of four suffering from disease.

At every military barrack door, as my friend Dr. Harry Thompson assured me, there lie in wait by the score prostitutes of the lowest and filthiest type, who pounce upon the recruits leaving duty and almost force the unhappy boys to their loathsome embrace, and yet our wise and benevolent statesmen know this; they also know how to stop it, and nevertheless are guilty of the daily ruin of hundreds, who get infected with syphilis, and invoke the curses of a future generation.

DR. ALFRED PARSONS expressed his entire concurrence with Dr. Falkiner's view that the only certain diagnosis of phthisis was to be made by a microscopical examination of the sputum; consequently the first step towards the isolation of tubercular patients must be the appointment of a public bacteriologist, whose duty it would be to examine the sputa of all suspected cases. Complete isolation, owing to the great prevalence of the disease which is answerable for about one-seventh of the total mortality, did not seem to him to be feasible, and as the greatest mischief was probably done by the expectoration of tubercular sputum on the public highways, he did not think much good would result from the compulsory isolation of patients who were so far advanced in phthisis as to be confined continually to bed. He would look rather to the education of public opinion than to legislative measures as a means of preventing infection, and he trusted that the day was not far distant when an enlightened public would provide sputum receptacles in places of resort, as is the custom on the Continent, and insist on phthisical patients carrying with them small spittoons for their expectoration instead of casting it broadcast. With reference to the notification of primary syphilis, he regretted that Dr. Falkiner had confined his observations to that sex which seemed to him rather sinned against than sinning. He strongly deprecated the introduction of any measures which would pander to a lax public morality, or minimise the risk attached to a wilful and avoidable transgression of nature's laws. He hoped that if notification of primary syphilis ever became compulsory, it would refer equally to each sex.

DR. C. F. MOORE said Dr. Stokes dwelt on the frequency of infection of the wife from the husband and the reverse in his lectures

at the Meath Hospital. In addition to separation of the infected, the prevention of phthisis by better drainage of the surface, as seen in the case of Salisbury, ought to be striven for. The carriage of phthisical sputa in a dry state may be one reason why phthisis is found not uncommonly in a dry country like Egypt. The occurrence of phthisis in parts of Ireland where the surface is damp or the dwellings of the people demands attention. In certain garrison towns, as Valetta and Hong-Kong, separation of the unfortunate class of females is or was carried out. The reinforcement of the Contagious Diseases Act would be a most important and valuable measure.

DR. J. W. MOORE agreed with the author of the paper that both tuberculosis and primary syphilis, being infectious diseases, should be liable to notification under the Notification of Infectious Diseases Act. Unfortunately, however, that Act was permissive—not compulsory, and at present there was endless confusion owing to the adoption of the provisions of the Act in some districts and its non-adoption in others. Dr. Moore instanced the existing unsatisfactory distribution of notification in Dublin and its suburbs—the Act was in force in the city and in the Pembroke township, it was not in force in the Rathmines district. He had also reason to know that even in the city, notification was very incompletely carried out. For example, in 1891, only 20 per cent. of the cases of enteric fever which occurred in Dublin city were notified. It was now proposed to add tuberculosis and primary syphilis to the list of notifiable diseases, while the Corporation had just arranged so to add measles and whooping-cough. This was all quite right, provided notification was insisted upon, and the penalties for non-compliance with the Act were enforced. Dr. Falkiner suggested that a recommendation should be made to the Local Government Board for Ireland to make tuberculosis and syphilis notifiable, but the Act left the question to the local sanitary authorities—not to the Local Government Board.

MR. MONTGOMERY considered that the present state of the Act as regards syphilis is very unfortunate, as any young man coming up from the country is at once pounced upon by some of those miserable creatures. Some time back the Act was in full force and acted well. One strong objection was that sometimes respectable females, innocent, were interfered with. As to tuberculosis, it was doubtful whether it should be notified, as it might lead to much annoyance by separation from families.

MR. EDGAR FLINN thanked Dr. Falkiner for his paper. Possibly, as Dr. Moore said, Dr. Falkiner had brought on the question of the necessity of the notification of tuberculosis and syphilis prematurely, but in doing so he showed his desire to see a much-needed reform added to the notification of diseases without delay. The Notification of Diseases Act was much too permissive in its character, and if the Bill now before Parliament of Local Government for Ireland passed in its present form, the grievance they all so much complained of would be made much worse in so far as notification was concerned. Tuberculosis and syphilis were just as necessary to be notified as the other scheduled diseases.

The PRESIDENT (Dr. MacDowel Cosgrave) suggested that if primary syphilis had to be notified, men might be likely to hide their disease and not apply for treatment, and the chief part of the cases notified would be those where the sufferer did not know the nature of the disease. Statistics are certainly required of phthisical cases, but it would be hard to get these by notification, as the same case might be notified time after time by different practitioners. The difficulty of segregating cases is almost insurmountable. Not only is it hard to separate cases in their homes, but early cases, still able to work, must spread the disease in stuffy workshops, and yet they cannot be kept from working.

SOME MODERN METHODS OF SEWAGE TREATMENT.

By D. EDGAR FLINN, D.P.H., F.R.C.S., M.R.C.P.;

Examiner in State Medicine, Royal Colleges of Physicians' and Surgeons'
Conjoint Board;

Formerly Medical Officer of Health, East Staffordshire.

[Read in the Section of State Medicine, May 6, 1892.]

Dr. Alfred Carpenter has truly said that—"It is the object of the sanitarian to bring about conditions which diminish the frequency of early death, which assist to remove bodily pain from the list of human grievances, and to pave the way for that time when every person born into the world will enjoy his birth-right, without having it curtailed by the acts of other people."

THE object I had in view in the drafting of this paper was to draw the attention of this Section to the benefits derivable from the most recent methods of sewage disposal. "The sewage problem," as it has been aptly termed, has passed through many stages during the past twenty years or more, but it is only within the last few years that scientific knowledge has sufficiently advanced to bring this vexed question within the possibility of a final and successful solution. Sanitarians have devoted themselves assiduously to grapple with the difficulties that beset their way in the disposal of the sewage of cities and towns, and if they had not, to a great extent, surmounted these difficulties, the condition of the large centres of population would long ere this have become intolerable. Further, in regard to the progress that has been made in sanitary science, the reign of Queen Victoria will ever stand prominently forth as a most important epoch, as at the time of her accession the sanitary condition of the people was as bad as bad could be.

There has been a rapid advance in the question of sewage treatment during the last ten years, and I opine that we have not yet come to a finality in this regard. The enforcement of the provisions of the Rivers Pollution Prevention Act of 1876 has become much more stringent, and effluents must attain a much higher standard of purity than heretofore. Previously to 1876 sewage matter and refuse from manufactories were discharged into rivers and streams, and the result was a wholesale pollution of the water-courses throughout the country. For some time after the Act mentioned above was passed, there was a great difficulty in the enforcement of its provisions, and it is only in very recent years that the benefits intended by its enactment have commenced to take effect. The new methods of sewage treatment have, no doubt, helped very much to this end, and the authorities have become much more alive to the necessity of preserving the waterways pure and wholesome. The alacrity with which sanitary reformers, as recently as twenty years ago, were anxious to get rid of refuse by pouring it into the nearest watercourse, completely blinded them to the fact that they were only temporarily relieving themselves of an evil that must, sooner or later, have its pernicious results elsewhere.

Three principal methods are recognised and are in use for the purification and disposal of sewage—viz., precipitation, filtration, and irrigation. The precipitation process is carried out by means of various reagents—the principal object in view being the purification of the sewage—lime, either alone or in conjunction with other chemicals, being most frequently used. The process of precipitation by lime has been pronounced, by the Rivers' Pollution Commissioners, "to be a conspicuous failure;" indeed, practically, the same may be said of the majority of the other methods employed in the precipitation of sewage, notably the lime

and sulphate of alumina process; the A. B. C. (alum, blood and clay) process; the lime and chloride of iron process, &c. The defined position of precipitation of sewage may be summed up in the words of Dr. Dupré, F.R.S., a well-known authority on sewage purification. He says: "As regards processes of precipitation I will merely remark that inasmuch as no proportion of chemicals which can practically be employed will do much more than clarify the sewage, the proportion of chemicals employed should be kept as low as is consistent with the object to be attained—namely, clarification; and that, more particularly, the use of large quantities of lime should be avoided." In the more modern process, known as "the ferozone and polarite process," which will be subsequently referred to, the introduction of the precipitating material ferozone is followed by filtration of the effluent through a filter containing polarite. The advantage of having a precipitant immediately followed by a filtering medium is obvious.

The filtration system of sewage purification is divided into simple filtration. and intermittent downward filtration. In the former process the sewage is merely strained or screened, but the effluent is not purified. In the latter process, that of downward filtration, the Rivers' Pollution Commissioners reported that no system was attended with better results than the filtration of sewage through a considerable depth of soil. According to the Report of the Commissioners this process of sewage purification "was essentially one of oxidation, the organic matter being converted into carbonic acid, water, and nitric acid; hence the necessity for the continual aëration of the filtering medium which was secured by intermittent downward filtration." The requisite filtering area required to carry out this process of purification, as estimated by the Commissioners, is one acre, drained to a depth of six feet for every 3,300 of the population.

The process of irrigation on sewage farms is the one attended with most beneficial results, as it not only secures a purified effluent, but, in most instances, becomes a source of profit, and utilises the value of the sewage for the benefit of the crops grown on the land; but to insure success the surface must be irrigated on the intermittent system to allow of sufficient aëration of the soil, and no doubt the best purifying results will be obtained by a combination of both methods—viz., irrigation and intermittent downward filtration. This system is in operation at Birmingham, Wolverhampton, Leamington, Reading, Doncaster, Croydon, Nottingham, Warwick, and other smaller towns.

The comparative merits of the systems of purification of sewage are given by the Rivers' Pollution Commissioners, as follows:—

	AVERAGE RESULTS.		
	Percentage of Dissolved Organic Pollution removed		Percentage of Suspended Organic Impurity removed
	Organic Carbon	Organic Nitrogen	
Chemical processes	28·4	36·6	89·8
Intermittent downward filtration -	72·8	87·6	100·
Irrigation -	68·6	81·7	97·7

The systems that have been thus briefly referred to are, no doubt, familiar to many, but the comparatively new systems of sewage treatment that I am desirous of bringing before this Section of the Royal Academy of Medicine are yet not very generally known, and in view of the fact that large and costly works are about to be carried out for the sanitary well-being of the City of Dublin, a short reference to each may thus be opportune. By way of preface it is, perhaps, worth noting that in connection with the London Metropolitan sewage disposal problem a combination of lime and proto-sulphate of iron was recommended by Mr. Dibdin, F.C.S., F.I.C., in the proportion of 3·7 grains of lime, in solution, to 1 grain of proto-sulphate of iron per

gallon of sewage matter. The Metropolitan Board submitted the initiative results of this process to four independent chemists of the highest standing—viz., Sir Frederick Abel, Dr. W. Odling, Dr. A. W. Williamson, and Dr. A. Dupré, who advised that the method of precipitation was a good one, but that it left a sufficiently unpleasant smell to prohibit the effluent water being discharged into the river during warm weather at all states of the tide; and they subsequently recommended, after careful observation of the experiments, that if the liquid, resulting from precipitation with lime and proto-sulphate of iron, was subsequently treated with manganate of soda and sulphuric acid, it would be deodorised and purified to such an extent as to render its discharge into the river Thames unobjectionable at all states of the tide—the resulting sludge being conveyed into deep sea by specially-designed steam barges. My object in making this reference is that in the proposed Main Drainage Scheme for Dublin a similar treatment of precipitation, and carriage to sea, of our metropolitan sewage will be carried out from the plans of Mr. Chatterton, C.E. I am not aware what the precipitant agent is likely to be, but the probability is that the London system may be followed in Dublin in its entirety, and, if such be the case, a final and successful solution to what has been the eye-sore of a century or more will be achieved.

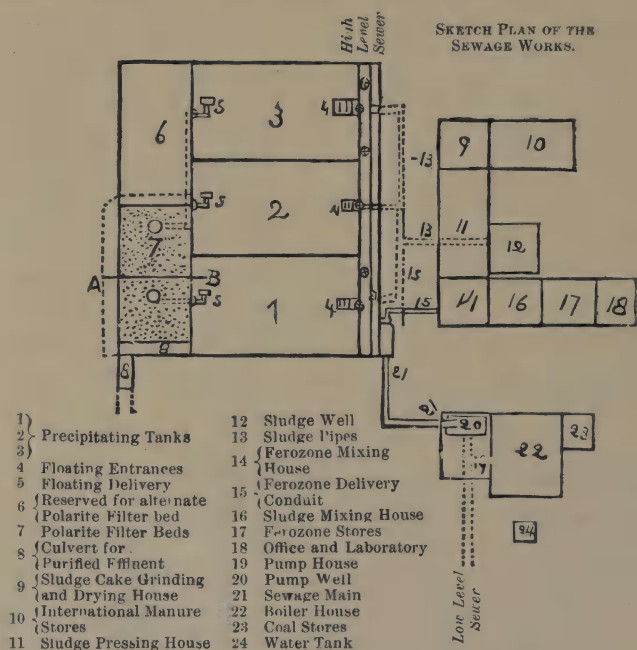
THE INTERNATIONAL SYSTEM,

or the Ferozone Polarite System of Purification is of very recent date, and where it has been in operation appears to have been most satisfactory, and its results have been very encouraging. It is quite a new departure in sewage treatment, and has excited a considerable deal of interest. It has been in use at Acton for the past four years, and the official reports credit it with being as perfect a system,

if not the most perfect, of sewage purification as has yet been devised, the purified effluent being inodorous, non-putrescible, clear, and tasteless, which can be discharged into a river or water-course without any danger, or any tendency to undergo secondary decomposition. The process is carried on in two stages, viz. :—

1. By precipitating and deodorising the sewage in settling tanks by the aid of a magnetic precipitant and deodorant called ferozone.

2. By passing the partly purified sewage effluent from the settling tanks through polarite filter-beds, which arrest any solids remaining in suspension, and oxidise and render innocuous the putrescible matter held in solution.



SKETCH PLAN OF THE ACTON SEWAGE WORKS, AND SECTION OF THE POLARITE FILTER BEDS.

Ferozone is the registered trade name of the material used in deodorising sewage, and precipitating the solids therefrom. It is rich in salts of iron, alumina, and magnesia, and also contains magnetic oxide of iron, in a very spongy and absorbent condition; by virtue of its soluble iron and magnesia salts it quickly causes subsidence of the suspended solids.

Polarite is the registered name of the material used for further filtering and purifying the ferozone sewage water from the putrescible matter dissolved therein; it is a black, porous, and magnetic oxide of iron, insoluble in water, and practically everlasting; it is very hard and absorbent; although consisting chiefly of iron, it does not rust, and its durability is unlimited; a cubic yard of polarite weighs about one ton, and is sufficient for rather more than six square yards of filter-bed; moreover, it is cheap enough to bring it into general use.

In carrying out the process of purification by this system, the sewage on reaching the outfall works is run through strainers to arrest floating solids, such as corks, rags, &c., and then flows quickly through a floating trough into a settling tank, the floor of which inclines towards the centre, so that a gutter may convey the sludge to the outlet valve. Before entering the tank the crude sewage receives a dose of ferozone which costs about one farthing for every thousand gallons of sewage treated. This can be added automatically by placing baskets of ferozone in the flowing sewage or by using Beloe's patent automatic sewage-mixing machine.

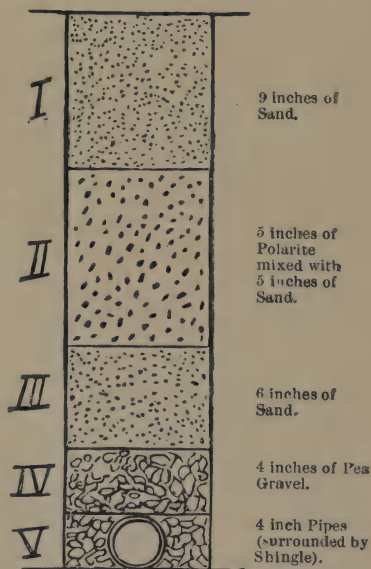
In places where the sewage does not flow by gravitation, but has to be pumped, and steam-power is therefore available, the ferozone may be ground with water or sewage in a small edge-runner mill and added automatically to the crude sewage in a liquid state. The flow of sewage through the tanks should not be continuous if it can be avoided.

Quiescent tanks are better for precipitation purposes, whatever kind of precipitant be used.

When a tank is full its contents should be left standing for about two hours, so that the ferozone may have time to act thoroughly. As towns differ in the quality of their sewage, the needful period of quiescence may vary in different places. The ferozone will cause deodorisation and precipitation to take place in the sewage, and a considerable part of the albuminoids in solution will coagulate and be precipitated with the solids. The supernatant sewage-water thus clarified would then be drawn off and run through polarite filter-beds which produce a higher degree of purification.

The polarite filter-beds usually consist of six inches of broken stone in which small agricultural drain-tiles are embedded, three inches of gravel, six inches of sand, twelve

SECTION ON LINE A.B. OF POLARITE FILTER BED.



VERTICAL SECTION OF "POLARITE" FILTER BED.

inches of polarite and sand mixed in equal proportions, and at the top a layer of nine inches of fine sand, making a total depth of three feet of filtering material. The depth of the layers is sometimes varied to meet special requirements. Polarite filters purify tank effluents which have been treated with ferozone at the rate of 1,000 gallons per square yard in 24 hours. They give better results than are generally produced by land filtration, and at less cost and without nuisance.

Land filters about $1\frac{1}{2}$ gallons per square yard in 24 hours, therefore one acre of polarite filter-bed will do more effective work than 666 acres of land. Hence the cost is much less than that of a sewage farm, and at the same time the results produced are more certain. The value of polarite for sanitary purposes can scarcely be over-estimated. By using it in small but powerfully active filter-beds it is unnecessary to buy large areas of land for sewage farms which often become a public nuisance.

Land is unable to take a continuous supply of sewage without great deterioration in its purifying and aërating properties. It gets clogged and choked with albuminous and glutinous matters, and becomes sewage-sick and inefficient as a purifying agent. Sewage comes every day to be treated whether the farm is, or is not, in a condition to deal with it. In wet seasons, when the farm is sodden by rain and wants rest, an increased quantity of sewage comes to be purified.

The polarite filter-bed only requires a few hours' rest occasionally for aëration and for cleansing the surface sand, which can be done by an improved method at a nominal cost. The polarite never requires to be removed or replaced. Hence it is better to lay down several small beds rather than one or two large ones, so that they may rest a few hours alternately for aëration and cleansing.

The resulting ferozone sludge is comparatively inoffensive in odour, and in this regard has an advantage over the lime, and other processes; it has been proved to be richer in manurial value, and the sewage manure produced by the process is shown by analysis to contain nitrogen equal to from one to two per cent. of ammonia, besides phosphates; if placed under cover it dries very quickly, and can be ground into powder. At Acton it is sold as a fine powder at the large sum of 30s. per ton, whereas in districts where lime is the precipitant the resulting sludge is carted away as a worthless material. Certainly this system of sewage treatment bids fair to solve a difficult problem, and if the resulting sludge can be made to return a fair revenue, I apprehend that urban and rural sanitary authorities will not be slow to recognise its merits. In Ireland it has not had a trial, but it is in operation at Hendon, Swinton, near Manchester, Balmoral, and sewage works are in process of construction at Guildford, Birkdale, and Sale.

As a filtering medium, polarite appears to have undoubted advantages as compared with sand-filtering material, and it is recommended for filter-beds in water-works, on the ground of economy as well as efficiency. It is well known that sand filter-beds have no chemical purifying power; they merely act as strainers, and retain putrescible matter, instead of oxidising and making them innocuous, and they leave them unchanged to contaminate the water. An instance of this occurred in the north of England, where an outbreak of typhoid fever was caused by the impurity of the filter-beds at the water-works. A Government inquiry traced the outbreak to fever germs in the filter-beds, and the epidemic did not cease until the filter-beds were closed. Polarite not only enters into the purification of sewage effluents, but is now being used in the laying

down of filter-beds in various parts of England, and report speaks highly of it as a filtering medium.

THE TREATMENT OF SEWAGE BY ELECTRICITY

is the most recent of all methods, and may be said to be very promising. Mr. Santo Crimp, referring to the Webster electrical process for sewage purification, says: "When it is considered that the application of electricity is almost daily extended to new objects, and that the science is as yet in its infancy, it must be admitted that the results achieved by the Webster process are of the most encouraging nature. The time may come when our towns will be lighted with electricity by night, whilst by day the dynamos will be employed in purifying the sewage; then the loading of the sewage with chemicals, with the consequent production of large masses of sludge, will no longer be necessary." Sir Henry Roscoe, M.P., F.R.S., has carefully investigated this process, and he mentions that "the quantity of sewage operated on in each experiment was about 20,000 gallons. The reduction of organic matter in solution is the crucial test of the value of a precipitating agent, for unless the organic matter is reduced the effluent will putrefy and rapidly become offensive. I have not observed in any of the unfiltered effluents from this process, which I have examined, any signs of putrefaction, but on the contrary a tendency to oxidise. The absence of sulphuretted hydrogen in samples of unfiltered effluents, which have been kept in stoppered bottles for three weeks, is also a fact of importance. By this process the soluble organic matter is reduced to a condition favourable to the further precipitation by natural agencies." Mr. Alfred Fletcher, F.C.S., F.I.C., Inspector under the Rivers Pollution Prevention Act, states:—"The result of my examination of this process has been to convince me of its efficiency in clarifying sewage,

of removing smell, and in preventing putrefaction of the effluent. I am of opinion that such effluent as I saw at Crossness can be discharged into a river, or after passing through a thin layer of sand, even into a stream, without causing any nuisance."

Analyses of Samples of Sewage treated by Webster's Electrical Process.

PARTS PER 100,000.

Description of Sample	Oxygen consumed	Free Ammonia	Albuminoid Ammonia	Chlorides	Notes
(A) Mixture of 3 samples of raw sewage collected during time of treatment	2.292	1.9	0.65	44.4	Stank badly on 3rd day
(B) Mixture of 3 samples of treated sewage flowing into tank before settling	0.917	1.75	0.6	37.2	Faint smell on 5th day
(C) Same as sample A after settling 24 hours	—	1.64	0.4	—	Stank badly on 3rd day
(D) Same as sample B after settling 24 hours	—	1.84	0.12	—	Faint smell on 5th day
No. 7. Average sample of raw sewage under treatment	2.5	1.8	0.65	41.2	Stank badly on 3rd day
No. 8. Treated sewage after 2 hours' settlement in tank	0.583	1.08	0.07	35.8	Perfectly sweet after 10 days
No. 9. Same as No. 8 after filtration through 6 in. of sand	0.536	1.00	0.06	35.6	ditto
No. 10. Treated sewage after 3½ hours' settlement in tank	0.5	1.28	0.05	35.6	ditto
No. 11. Same as No. 10 after filtration through 6 in. of sand	0.5	0.91	0.01	35.6	ditto

Probably low as the samples had been kept

The effluent produced by the electrical process, Mr. Webster states, contains about three grains per gallon

of suspended matters, which consist almost entirely of oxide of iron, which is quite innocuous. Where this may be objectionable, from a sentimental point of view, it can be entirely removed by filtration through a few inches of sand.

The bacteria question is one which has probably still to be settled, but, in order to obtain some information as to the action of the iron compound produced by electro-chemical decomposition, some experiments were carried out, with the result that, after a given treatment, the whole of the bacteria were killed. In the case of experiments carried out in Paris, with ordinary treatment by means of iron electrodes, the results were as follows:—

	Raw Sewage	Effluent
Organisms per cubic centimetre,	5,000,000	600

In another experiment, in which the effluent was treated still further, all organisms were destroyed.

Unquestionably this process of sewage treatment has many advantages, and may yet solve the vexed sewage problem.

Mr. Webster remarks further, "That it was while working with perchloride of iron, as a purifier of sewage, that the importance of iron salts, in relation to organic matter, became apparent; and it was in connection with this chemical and free chlorine gas that the idea of electrolysis suggested itself."

THE "OXYGEN PROCESS."

A new process, which has had its birth in Ireland, and which promises to fully realize the expectation of its promoters, is the "Oxygen Process of Sewage Purification," devised by Mr. W. E. Adeney, F.I.C., and Mr. W. Kaye Parry, M.A., C.E. It has been submitted to an exhaustive investigation both in the laboratory and at some works at Monkstown, and the results have been successful. The pro-

cess, moreover, has been examined in detail by the Chief Surveyor to the Board of Public Works, and has been selected by that body for the purification of the sewage of the Lunatic Asylum at Dundrum. To be brief, the results effected by this process are the complete separation of all matters in suspension in sewage, in a state suitable for air drying and utilisation as a manure, and an effluent of any degree of purity according to the circumstances of the locality to which it is applied may demand, one of the most interesting features in the process being the separation of the solids, *without the addition of precipitating chemicals.*

In this process the solid matters in the sewage are first separated from the soluble constituents by subsidence in a tank of special construction, no precipitating chemicals being employed. The destruction of the fermentable organic matters in solution in ordinary sewage is effected by taking advantage of the power which harmless micro-organisms, the germs of which are known to be widely distributed in the earth, air, and water, have, when growing under the most favourable conditions, of rapidly decomposing such matters into carbonic acid, water, ammonia, and also nitric acid. Under proper conditions these products result directly from the decomposition of the soluble fermentable matters in sewage, without any intermediate putrefactive fermentation being set up. The formation of offensive bodies is, in fact, entirely avoided in this process.

The essential condition for the healthy and rapid action of the organisms here referred to, is a plentiful supply of oxygen, free or combined. The necessary supply of oxygen is secured by a careful use of nitrate and manganate of sodium. One of the products of decomposition of the latter chemical is oxyhydrate of manganese; it is completely recovered by subsidence in a second tank, similar in construction to the one employed for the separation of the

solid matters of the sewage. The recovered oxyhydrate of manganese may be dried and re-converted into manganate of sodium, and so may be used over and over again. It may also be employed for mixing with the solid matters of the sewage separated in the first tank to prevent putrefactive fermentation being set up in them during the process of air-drying them.

Another system,

THE SHONE HYDRO-PNEUMATIC SYSTEM,

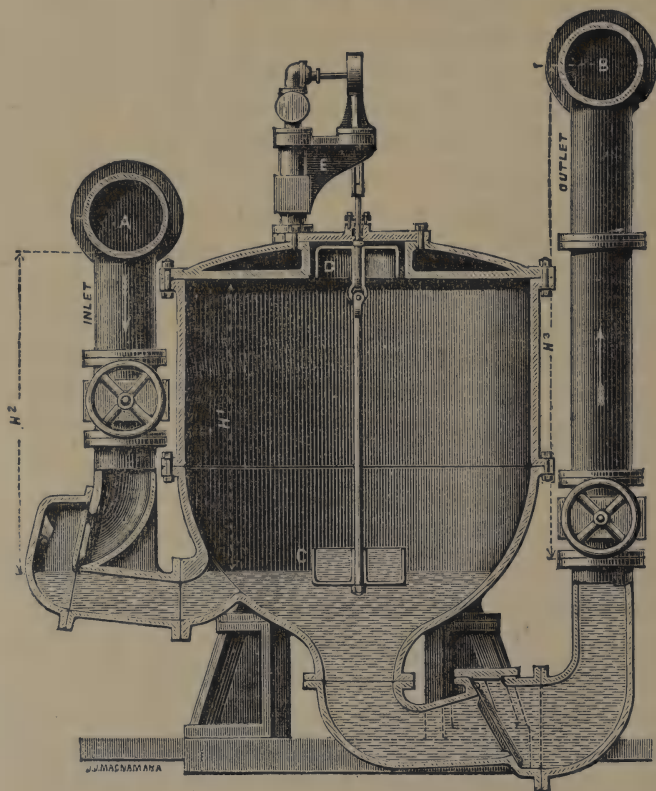
is also of comparatively recent date, and is in operation at Eastbourne, Shirley, and Freemantle, near Southampton, Lowestoft, Stoke-on-Trent, and some other thirty or forty towns in England, as well as at the Houses of Parliament. The system appears to work admirably, and is particularly adapted to districts and towns of a low-lying nature, and where the volume of sewage is insufficient to cause what is termed a self-cleansing velocity in the sewers. Correctly speaking, the Shone System might be better described as a successful method of getting rid of sewage by means of automatic sewage ejectors, and a writer described it some time since in the columns of *The Health Record* as a system so elastic in its application that it can not only be successfully applied to a solitary building, but is equally adaptable to towns, cities, and rural districts.

In the subjoined illustration, the working of the Automatic Sewage Ejectors is shown, and is carried out as follows:—

The sewage gravitates from the sewers through the Inlet Pipe A into the Ejector, and gradually rises therein, until it reaches the underside of the bell D. The air at atmospheric pressure inside the bell is then enclosed, and the sewage continuing to rise outside and above the rim of the bell compresses the enclosed air sufficiently to lift the

bell, spindle, &c., which opens the Compressed Air Admission Valve E. The compressed air thus automatically admitted into the Ejector presses on the surface of the sewage, driving the whole of the contents before it through the bell-mouthed opening at the bottom, and through the Outlet Pipe B into the iron sewage rising main or high level gravitating sewer, as the case may be. The sewage can only escape from the Ejector by the outlet pipe, as the instant the air pressure is admitted on to the surface of the fluid the valve on the Inlet Pipe A falls on its seat and

AUTOMATIC SEWAGE EJECTOR.



prevents the fluid escaping in that direction. The fluid passes out of the Ejector until its level therein reaches the cup C, and still continuing to lower, leaves the cup full until the weight of the liquid in the portion of the cup thus exposed and unsupported by the surrounding water is sufficient to pull down the bell and spindle, thereby reversing the Compressed Air Admission Valve, which first cuts off the supply of compressed air to the Ejector, and then allows the air within the Ejector to exhaust down to atmospheric pressure. The outlet valve then falls on its seat retaining the liquid in the sewage rising main; the sewage then flows into the Ejector through the inlet once more, driving the free air before it through the air valve, as the sewage rises, and so the action goes on as long as there is sewage to flow.

The valuable feature connected with the system consists in this—that by its aid the “separate system” of drainage can be carried out in the most perfect and sanitary manner possible, whether the district to be drained be flat, low-lying, tide-locked, or otherwise. All the chief sanitary engineers of the present day recommend the adoption of the separate system in preference to the combined system. But towns drained on the separate system now are few and far between, the reason being that the engineers who have designed the works could never get the hydraulic conditions necessary to cause the sewage to flow freely to what are called the out-falls; because it is well known that small volumes of sewage require to be turned into small sewers, having inclinations suitable to their sizes; and in flat districts the necessary inclinations can never be obtained.

In those districts hitherto drained by gravitation *per se*, the practice has been to construct big sewers at such gradients or inclinations as the configuration of the country or town would admit of. In dry weather, when only the

sewage proper is discharged into such sewers, the volume is insufficient to cause what are called a self-cleansing velocity in the sewers, which then become sewers of deposit, and the large spaces within them not occupied by the sewage are more or less filled with foul air mixed with sewage gases, and these are permitted to escape haphazard into the streets under which they are laid, and into the houses with which they are connected.

In this manner unsanitary, and not sanitary, drainage works have been, and still are being, executed all over the world. But by the Shone System the most perfect hydraulic gradients can be secured throughout the whole area to be drained. Besides securing self-cleansing sewers by the Shone System, these sewers can be effectually ventilated by the action of the ejectors in a most simple but thoroughly scientific and practical manner.

It will be noted that the modern or later attempts to solve the sewage difficulty possess many features of practical interest, and I claim the indulgence of this Section if I have unduly trespassed on its time.

PROFESSOR CHARLES TICHBORNE said that Mr. Flinn's paper was of great interest, because it brought before them the various processes which had lately been tried for sewage treatment. The great difficulty of all methods of sewage disposal was that this difficulty grew directly as the size of the town they had to deal with. Therefore every town required its special treatment, and he did not believe that any universal system was applicable. Some years ago he, the speaker, had been taken by Mr. Dibdin, Chemist to the Metropolitan Board, to see the method adopted at London. The process at that time was a very simple one. It consisted in carrying down the sewage by two main sewers into settling tanks, pumping the effluent into the river, and taking away the sludge by steam barges and emptying it into the sea. He was told that even at that time the great difficulty they had to contend with was to dispose of the sedimentary sludge quick enough. How much

greater would that be with the monster city where they were using precipitants. The effluent presented no difficulty. A small proportion of manganate of sodium was added before pumping for the purpose of destroying effluvia. The interesting process mentioned by Mr. Flinn, in which magnetic oxide of iron played so important a part, seemed to Prof. Tichborne to be based on a similar theory to the porous iron filters which had been found so successful. There was nothing new about the use of manganates and permanganates for sewage purification. He should like to hear how far the irrigation system was carried out at Birmingham. Birmingham was a very large town, and if Mr. Flinn could tell them something about that, it would be very interesting.

DR. ALFRED PARSONS regretted that Mr. Flinn had devoted such a large part of his paper to an account of the chemistry of the methods of sewage treatment, while the bacteriological aspect of the question had been dismissed in a few brief sentences. To him the latter seemed to be the all-important side of the subject. He yielded to none in his respect for the science of chemistry. It had had a glorious record in the past, and there was a brilliant future before it, but he maintained that the value of any method of sewage treatment must be weighed in the balance of bacteriology and not of chemistry. Numerous opinions of distinguished chemists, from Sir Henry Roscoe to Professor Adeney, had been laid before them, and the most they appeared able to say of any system was that the effluent was colourless, tasteless, odourless, but a fluid, he believed, might possess these characters and yet contain large numbers of typhoid bacilli. A liquid, swarming with pathogenic organisms, which from its odour, taste, and appearance is repulsive, is likely to do less injury than a fluid containing similar organisms, but commendable from fulfilling those criteria on which the chemists appear to lay so much stress. Till an exhaustive bacteriological examination of the fluid after precipitation has been made, he must look with great suspicion on any method of treatment in which the effluent is poured into superficial drains which in their course may contaminate many sources of water supply. The use of the sludge for manuring vegetables appeared to him likewise to be fraught with danger, and it did not seem to him to require any vivid imagination—in the absence of any information in the paper of the bactericidal properties of the substance used in the process of precipitation—to understand how typhoid fever might be propagated by the minute particles adhering to imperfectly washed, uncooked

vegetables such as lettuce, assuming the presence of typhoid dejecta in the original sewage matter.

DR. CHARLES MOORE asked Mr. Flinn what was the "Shone System," and if it could be carried out in detail without forming the great aggregate of sewage now so generally the result of main drainage systems. The great difficulty of such systems being the vast amount of liquid matter that has to be dealt with. The prevention of contamination of sewers by the matter from the 80 slaughter yards about Dublin and from other well-known sources of contamination should be insisted upon. The pumping of the surface water by the old pumps of Dublin was an important measure of prevention of many diseases—as phthisis, &c. The larger the system of main drainage the less surface drainage pumping by steam works.

DR. MACDOWEL COSGRAVE asked for the comparative expense of the Shone process.

MR. FLINN replied.

SECTION OF ANATOMY AND PHYSIOLOGY.

A FEW APPLICATIONS OF A PHYSICAL THEOREM TO MEMBRANES IN THE HUMAN BODY IN A STATE OF TENSION.

By ROBERT H. WOODS, M.B., DUBL.;

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[Read in the Section of Anatomy and Physiology, January 15, 1892.]

It will first be necessary to state the theorem which I intend to apply.

On a section of a cylinder of radius r containing fluid under a pressure of P lbs. per square inch let us take two diametrically opposite points A and B , and find the tensions per inch run of the cylinder at these points.

The total pressure acting on the semi-circumference of a ring an inch wide will be $P\pi r$. But the stress or tension at A and B is not represented by this total pressure, but by the sum of the resolved parts of its components in a direction at right angles to the diameter AB . The sum of these components is equal to the pressure on an area formed by the diameter of the cylinder multiplied by unity or $= 2Pr$, but since an equal share is borne by A and B , the tension or stress at either point is:

$$T \propto Pr \text{ or } P \propto \frac{T}{r}$$

This is for the simple case of a surface curved circularly in one direction. If the surface be curved in a direction at

right angles to this, the second radius of curvature being r_1 , we have :—

$$P \propto T \left(\frac{1}{r} + \frac{1}{r_1} \right) \quad . \quad . \quad . \quad . \quad (a)$$

From whence it is seen that the tension of a membrane enclosing a fluid under a given pressure is not a fixed quantity, but is greater the greater the radii of curvature of the membrane. The truth of this may be put to a practical test by distending a large and a small bag of the same material with a fluid, the pressure of which is gradually raised. It will always be found that the larger of the two will be the first to burst, while the smaller will remain intact under a very much higher pressure.

If, now, we apply this to the heart, we see that the muscle of the heart in the distended state of the organ must, on account of the larger size of its cavity, and necessarily greater radii of its walls, make a much greater effort at contraction, in order, by means of the tension of the walls, to raise the contained blood to the pressure of that in the aorta, than when the ventricle is more contracted, as *e.g.* at the middle or end of systole.

Again, the thinner the heart-wall is—that is to say, the fewer muscle fibres there are on cross section—the more will each fibre have to exert itself to bring about a given tension; and when it is considered that the wall of the heart is thinnest when the organ is most dilated, it will be seen that this must form another and not unimportant factor in contributing to the difficulty of commencing systole.

Let us now go a little more accurately into these points.

If we apply the fact that the surface area of a sphere varies as the square of its radius, and bear in mind that the total number of muscle-fibres is a constant quantity, we see that the number of muscle-fibres per unit of area on the heart-wall varies inversely as the square of the radius—that

is, supposing for a moment the heart to be a sphere. Hence, for a given exertion of muscle-fibre the tension will vary inversely as the square of the radius, or

$$T \propto \frac{1}{r^2}.$$

Still regarding the heart as a sphere, the formula (a) above,

$$P \propto T \left(\frac{1}{r} + \frac{1}{r_1} \right)$$

now becomes

$$P \propto \frac{2T}{r}$$

But $T \propto \frac{1}{r^2}$ as just shown, therefore

$$P \propto \frac{1}{r^3}.$$

Thus we arrive at the somewhat startling result that the pressure varies inversely as the cube of the radius when the exertion of each individual fibre is taken as constant, or if we regard the pressure as constant, the exertion or pull of each muscle fibre must vary as the cube of the radius of curvature. So that if we suppose the heart to be in two conditions, the first condition having a certain diameter, and the second a diameter twice as great, the heart will have to exert itself eight times as much in the second position as in the first, in order that the same pressure may be exerted on the contained blood.

This enormous disadvantage under which the heart in the beginning of systole labours, is surely more than sufficient to demand extra mechanism to overcome it; and it now remains to be considered how it had best be done.

The difficulty could easily be disposed of by heaping on layers of muscular fibres outside those which were barely sufficient in the contracted condition; but this would be attended by the disadvantage of more required room, and of

having the extra fibres acting at a time when they were wholly unnecessary, *i.e.*, towards the middle and end of systole, which would not be in accordance with the principle of least action enunciated by Dr. Haughton in his great work on animal mechanism, and shown by him to be universal in the animal economy. Some cheaper way must, then, be sought. It is found in the columnæ carneæ, and, as I hope to show, in the muscoli papillares also. These muscles, which stretch across the cavity of the heart from wall to wall, exert their influence more immediately on the blood by pulling more directly on the ventricular wall, and consequently are more efficient than if they lay on the outside of the wall itself. On account also of the rapidity with which their origins and insertions approximate the one to the other, their force must diminish towards the middle, and fade away towards the end of systole; thus compensating in a singularly beautiful way for the disadvantage at which the heart-wall is placed at the commencement of its contraction.

It may be objected that placing muscles inside the heart cavity would necessitate its enlargement for the purpose of making room for them. But a little consideration will show this is not the case. For suppose the heart a sac with smooth walls, it would be quite impossible for it to contract so as to obliterate or even almost obliterate its cavity when expelling blood under considerable pressure, owing to the difficulty of approximating the origins and insertions of the fibres, especially of the innermost layers, so that the only difference their presence makes consists in diminishing the quantity of residual blood in the heart.

The urinary bladder is no exception to this, but rather an illustration. For the last quantity of urine is got rid of not by the exertion of the bladder wall, but by the *vis a tergo* of the abdominal muscles and the pumping of the accelerator urinæ muscle, aided to some extent by gravity. Here, too,

the resistance to be overcome is negligible, being nothing more than that offered by the friction of the urine against the walls of the urethra. The bladder, furthermore, as shown by frozen sections, is, in the contracted condition, not spherical, but somewhat T-shaped, which it could not be if its walls were in contraction at the end of the act of micturition.

The muscoli papillares are usually considered as having only to do with controlling the mitral and tricuspid valves. But they must also aid in expelling the blood. For the exertion necessary to prevent the valves from flapping back into the auricles must also react on the ventricular wall and help it in its effort at contraction. They must then be looked upon as having the double function of controllers of the valves and true working muscles of the contracting heart itself.

Another and a very interesting example of similar, though less complete, prominence or ridging of muscular fibres on the inside of a contractile sac, for the purpose of exerting greater pressure on the contained fluid, is exhibited as a pathological condition in the urinary bladder in cases where the outflow of urine is gradually resisted, as in the bladder of enlarged prostate. Here the innermost fibres raise themselves up from the wall-hypertrophy, and by their peculiar disposition increase their efficiency. I am inclined to regard this as an attempt on the part of the bladder to simulate the construction of the heart by providing, when the necessity arises, its own columnæ carneæ; and if this be so, it cannot fail to strike one as being an unusually elegant example of the wonderful resources of Nature when combating disease.

In cases of valve disease the heart becomes dilated to allow for regurgitation, in order that the quantity of blood thrown into the aorta may as nearly as possible be kept normal, and so the blood-pressure kept up. There is, however, reason to

believe that in spite of this the blood-pressure in most cases falls a little. Concomitant with dilatation we have hypertrophy. What is the cause of this hypertrophy, especially if the blood-pressure be lower than before? The reason is because the tension required in the ventricular wall to raise the blood, even to a lower pressure, is greater when the heart is pathologically dilated, and so its wall-curvature more gradual, than when of normal size, and the curvature sharper.

It is a well known fact that the apex of the heart is its thinnest part. The reason is simple. It is because here the curvatures of the wall are sharpest, and, as above shown, the tension required to resist or cause a given pressure in the ventricle will be less than where the curvature is more gradual. The same reason explains why, when the heart bursts, as in buffer accidents, without any part having been specially diseased, the thinnest part is not always chosen as the seat for rupture.

This question of tension in the walls of a sac containing fluid under pressure explains the absence from fusiform aneurysms and varicose veins of any effort to heal spontaneously; for the more the vessels dilate, the higher the tension in their walls rises, and the more incapable they are of contracting to diminish their calibre.

In fact, when we take into consideration the comparatively great radii of curvature of the sac of an aneurysm, and the consequently great height of the tension in its walls which the blood-pressure in its interior must give rise to, and bear in mind the diseased condition of its walls, we cannot help wondering that they do not more frequently burst than they are known to do.

Let us now for a moment regard the uterus in labour, and I think we shall see the real reason why letting a quantity of *liquor amnii* flow away precipitates parturition. It is

because this act diminishes the capacity, and so the radii of curvature of the uterine walls, and thus enables the organ to exert a greater pressure on its contents than in its former more dilated condition.

The “atony” of the uterus, where the quantity of *liquor amnii* is excessive, is, I believe, to be explained in the same way—that is, the too great curvature of the walls of the uterus,—and is not to be attributed to the overstretching of the muscular fibres; for it is not easy to conceive that the *liquor amnii* can increase so rapidly as not to give time for the muscular fibres to adapt themselves to the change. Whether this reason is the only one I am not prepared to say, but that it is a very important one no one can deny.

It will be admitted that the thickness of the heart at any place bears a direct proportion to the relative tension at that place. Hence it follows that in the equation (*a*) above, we ought to be able to substitute *t* the thickness of the walls, for *T* the tension. The equation will then be

$$P \propto t \left(\frac{1}{r_1} + \frac{1}{r} \right)$$

or if the pressure be constant, $t \left(\frac{1}{r_1} + \frac{1}{r} \right)$ will also be constant;

or, in general terms, the thickness varies as the radii of curvature.

I have experimentally put this to the test in the following way. Some hearts were obtained, and the auricles having been ligatured, the aortic and pulmonary valves were destroyed, and glass tubes tied into their orifices. The ventricles were then dilated with spirit under a head of about 12 in., and so left until they were hardened. Points on the ventricular surface were then selected and marked with labelled pins, and the curvatures in two directions at right angles to one another estimated for each point. The heart

was finally cut, and the various thicknesses at the marked points measured.

One heart was an adult's, another a child's about twelve months old. A third, adult heart, I compressed so as, as far as possible, to imitate the contracted condition of the organ, and see if here the relationship held; but in this case the measurements of the curvature were not so reliable, as it was difficult to make sure that the walls were uniformly compressed, and indeed I have reason to believe that I did not very well succeed.

I have tabulated the results (see below), and I think they show the theorem to be correct within the limits of experimental inaccuracy. It will readily be understood that these measurements are not very easy to take; for in the one case the curvature sometimes changes so rapidly that only a small line can be taken as a segment of a circle, and in the other case the attachments of the columnæ carneæ do not always leave it clear at exactly what point the true heart-wall stops and the columnæ themselves may be said to begin.

On the whole, however, the results more than realised my anticipations, and may, I think, be taken as proving the point.

There were two points taken on the left ventricle of the adult heart—one near the anterior, and the other near the posterior interventricular groove; but here the formula did not hold good, and the results were so much at variance with the rest that I had to omit them. This is confirmatory of the proposition; for it shows that where the conditions were disturbed by the proximity of the interventricular septum there, as might have been anticipated, the relation between curvatures and thickness did not hold.

Another point of interest is brought about by these tables, viz., that the pressure in the left heart is about three and a half times as great as in the right in the child's case, and

about six and a half times as great in the case of the adult.

This latter result, I have since found out, corresponds almost exactly with the experimental observations of Knoll on the rabbit. His exact proportion was 1:6·8. The proportion in the child's case is, I have reason to believe, not correct, as the ventricles were not in a similar condition.

There is good reason to believe that the pressure in the aorta of an adult is a little over 9 feet of water. This being so, the pressure in the pulmonary artery would be 17 inches.

This accounts for the fact that the left ventricle is more rounded on section, the mean curvature of its walls sharper, and their thickness greater than the right.

The left ventricle is the model of what a ventricle ought to be. The right could, I think, be improved upon. The work could be more cheaply done by having another (free) heart, consisting of a simple auricle and ventricle, instead of having one ventricle tacked on to the outside of another. For this would admit of the wall-curvature being sharper, and so the required tension smaller. Whether the exigencies of space and the shape of the organs would admit of such an arrangement I cannot say, for of course these elements, and perhaps others too, would enter into any plan of remodelling.

The thickness of the walls of the systemic arteries diminishes the further we go away from the heart, but more rapidly than the blood-pressure, the reason being that the bore also diminishes, and so the radius of curvature; and hence the required tension in the arterial walls diminishes proportionately.

ADULT HEART (*normal*).—Under pressure of 12 inches.*Right Ventricle.*

Label of point chosen.	r mm.	r_1 mm.	t mm.	$t \left(\frac{1}{r} + \frac{1}{r_1} \right)$
z_1	60	60	1.5	.050
c_2	65	80	2.0	.055
d_2	32	75	1.25	.055
a_2	75	90	2.2	.054
e_2	30	45	1.0	.055
f_2	55	90	2.0	.058

.0545 = average.

Left Ventricle.

p_1	36	60	8.0	.35
z	32	80	8.5	.37
x_1	70	36	9.5	.39
r_1	30	80	8.5	.38
o_1	28	60	7.0	.36
s_1	70	40	8.5	.33
m_1	80	40	10.0	.37
w_1	32	80	8.5	.34
f_1	55	16	5.0	.40
t_1	70	24	6.0	.33
v_1	24	70	6.5	.36
n_1	60	24	6.0	.35

.36 = average.

CHILD'S HEART (*normal*).—Under pressure of 12 inches.*Right Ventricle.*

Label of point chosen.	r mm.	r_1 mm.	t mm.	$t \left(\frac{1}{r} + \frac{1}{r_1} \right)$
e_1	50	22	2.2	.14
c_1	40	20	2.0	.15
h_1	18	55	2.5	.18
d_1	80	30	3.0	.14
i_1	60	20	2.0	.13
b_1	55	16	2.5	.20
a_1	80	24	3.0	.16
l_1	10	40	1.2	.15
g_1	6	18	.7	.15

.166 = average.

Left Ventricle.

<i>o</i>	18	22	6·0	·60
<i>n</i>	24	16	5·5	·57
<i>v</i>	38	22	7·5	·54
<i>l</i>	14	34	5·5	·56
<i>t</i>	50	14	6·4	·58
<i>q</i>	20	55	7·0	·50
<i>s</i>	12	36	5·25	·61
<i>p</i>	10	65	5·0	·58
<i>r</i>	12	22	4·5	·57
<i>w</i>	22	10	4·0	·58
<i>y</i>	9	18	3·0	·50
	—	—	—	—

·56 = average.

THE PRESIDENT (DR. BROOMFIELD) congratulated Dr. Woods on his able paper. He was glad to hear his conclusions as to the use of the muscoli papillares and columnæ carneæ, with which he agreed.

PROFESSOR FRASER doubted the applicability of such precise mathematical rules to structure in animal bodies. He would like to know if Dr. Woods' conclusions might be applied to all hearts.

PROFESSOR BIRMINGHAM complimented Dr. Woods upon the production of such an interesting paper. Applying these conclusions to the heart of man, he would like to hear from Dr. Woods how he accounted for the distribution of the columnæ carneæ on the interventricular septum. In the left ventricle, above, where the curvature is most open, the columnæ carneæ are almost absent, while they are numerous below, where the curve is reduced. On the right side of the septum, which is convex, the columns are much better developed. Professor Birmingham also referred to an old theory of the use of the columnæ carneæ—namely, that by whipping the blood, they caused it to give off its gas more readily, as when flat soda-water is whipped up, it gives a renewed discharge of gas. He much preferred Dr. Woods' scientific explanation.

In reply, DR. WOODS was of opinion that his theorem and conclusion might be applied to all hearts. He thought the soda-water explanation of the function of the columnæ carneæ could not be entertained, as the gases were not given off in the ventricle, and further, were not in true solution in the blood; and that the reason why stirring flat soda-water evolved more gas, was that it was shown the example by the gas adhering to the rod.

NOTE OF AN IRREGULAR DISTRIBUTION OF NERVES ON THE DORSUM OF THE FOOT.

By P. J. FAGAN, L.R.C.S. & L.R.C.P., &c. ;
Demonstrator of Anatomy, Catholic University Medical School.

[Read in the Section of Anatomy and Physiology, Jan 15, 1892.]

ONE of the subjects proposed last winter by the Anatomical Society of Great Britain and Ireland for collective investigation was the distribution of nerves on the dorsum of the foot. In all some 229 feet were examined and reported upon. Amongst these there were found 12 varieties in the arrangement of the nerves.

My attention was called recently by a student to a condition which seems not to have been represented amongst the 229 feet examined in the collective investigation, and which, perhaps, deserves recording, owing to the peculiar overlapping and anastomosis of nerves which took place, as well as the increased areas of supply of the internal and external saphenous, coupled with the diminished area which was innervated by the musculo-cutaneous nerve.

The external saphenous divided into two branches at the external malleolus: the outer of these ran undivided to the outer side of the little toe; the inner split into three twigs, which ran to the clefts between the four outer toes, where they again divided to supply the contiguous sides of these toes; thus the external saphenous supplied three and a half toes. In 24 per cent. of cases recorded in the collective investigation it supplied two and a half toes. The internal saphenous ran on to the inner side of the great toe. In one case only of the 229 referred to, it is reported to have done so.

The external branch of the musculo-cutaneous was, apparently, absent; the internal split into two—one went to the inner side of the great toe, the other anastomosed with the branch of the external saphenous at the cleft between the second and third toes. The anterior tibial divided into two branches—one split to supply the contiguous sides of the great and second toe, the other overlapped the anastomosis of the external saphenous and musculo-cutaneous, and was distributed to the dorsum of the third toe.

The whole is a marked example of the extreme variation which may be found in the nerve supply of cutaneous tracts. It is also an example of nerves invading the territories of their neighbours, and it illustrates well that overlapping which was shown to take place in the hand by Dr. Brooks some years ago in this Section, and which, in association with the anastomosis of nerves, accounts for the varying amount of anæsthesia found as a result of cutting certain nerves; for, as will be seen, the outer side of the little toe was supplied by one nerve, the inner side of the great toe by two nerves, and the inner side of the third toe by three nerves. Cutting a nerve going to these three parts would produce, of course, entirely different amounts of anæsthesia in each.

The PRESIDENT discussed the communication at some length. He referred to the abnormal arrangement of the anterior tibial, and he compared it to the condition which he had found in certain lizards. He had never seen an internal saphenous with such an extensive distribution.

EXTREME ANOMALY OF THE HEART AND GREAT VESSELS.

By AMBROSE BIRMINGHAM, M.D. ;
Professor of Anatomy, Catholic University, Ireland.

[Read in the Section of Anatomy and Physiology, March 25, 1892.]

FOR the specimen, the description of which follows, I am indebted to my friend Dr. Joseph O'Carroll, who had the case under observation for three years previously to death. Dr. O'Carroll supplies the following history :—

J. H., female, aged 20 years (occupation, dealer in fish), was of small medium stature, like the other members of her family; had long but clubbed fingers; was cyanotic in appearance. On examination she presented a loud, harsh, systolic murmur, audible from the 2nd *right* intercostal space downwards for 2 inches below *right* nipple, and into the *right* axilla. The second sound was loud, sharp, and accompanied by a beat in the 2nd right interspace, easily felt by the hand. Suffered from metrorrhagia when 15; got well after a time. Suffered from occasional attacks of dyspnoea, which passed away after rest in bed. At 18 got a new attack of metrorrhagia; brought to hospital in an exhausted condition; treated by curetting and iodine injections into the uterus; four days later got right hemiplegia. Imperfect separation of cavities of heart diagnosed. Recovered from hemiplegia in a few months. Returned a year later, suffering from grave dyspnoea and pain in the left side; died in a month. Post-mortem:—Abdominal organs normal; pelvic viscera smaller than usual; lungs tubercular, especially left. Heart and great vessels as described below.

I received the heart and lungs, with portions of the trachea, aorta, and oesophagus.

At first sight the case seems to be one of simple dextrocardia, or transposition of the parts of the heart, without any change in the condition of the viscera generally. As

pointed out above in the post-mortem examination, the lungs and abdominal viscera were anatomically normal, the chief anomalies being confined to the heart and great vessels. They may be here summarised:—The apex of the heart is directed to the right; the whole organ is twisted on its long axis, the left margin coming forward so that the anterior surface is formed by the two ventricles in almost equal proportions. The aorta comes off from the right ventricle in the position normally occupied by the pulmonary artery; it curves over the root of the right lung, forming a right aortic arch; the pulmonary artery arises from the same ventricle, its orifice being placed midway between base and apex among the columnæ carneæ, and its lumen being reduced to a diameter of nearly quarter of an inch. There is a large aperture, admitting a thumb, in the upper and anterior part of the ventricular septum; through this the two ventricles communicate freely. No artery arises from the left ventricle. There is only a trace of the septum of the auricles, the two cavities communicating with perfect freedom; the two auricular appendices lie side by side to the left of the base of the heart, and do not embrace the arterial trunks. The left superior cava is persistent and pervious.

On looking at the heart (Fig. 1) it is seen at once that the apex is directed to the right; and further, that it is formed by the tip of the right ventricle and the interventricular septum. The base is directed upwards and to the left. The left margin approaches the horizontal in position, and is apparently the margo-acutus, being much sharper than the right. The anterior interventricular groove is somewhat nearer to the left margin than to the right, while the posterior groove divides the posterior surface almost equally, lying perhaps a little nearer to the right than to the left margin. From the front, the right ventricle is the more evident; from behind, nearly equal portions of both are seen.

At the base on the right side, where the auricular appendix is normally found, no trace of an appendix can be seen; while on the left side there are apparently two appendices, placed side by side, to the left of the arterial trunks (R.A.A. and L.A.A.).

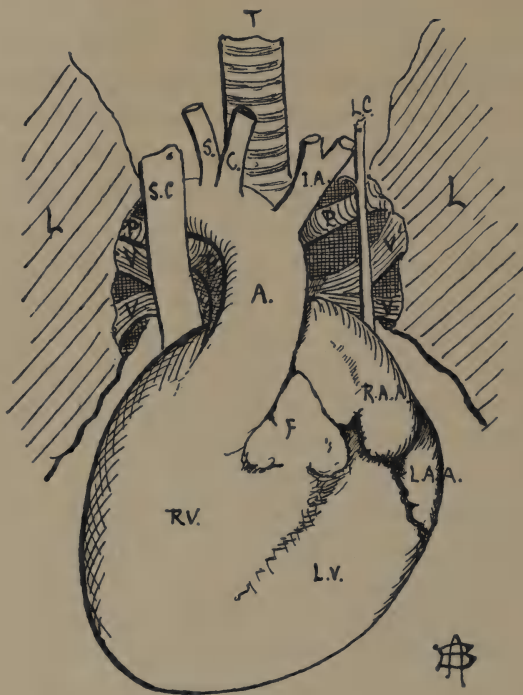


FIG. 1.—*R.V.*, Right ventric'le. *L.V.*, Left ventricle. *R.A.A.*, Right auricular appendix. *L.A.A.*, Left auricular appendix. *F.*, Mass of fat. *A.*, Aorta. *I.A.*, Left innominate artery. *C.*, Right carotid. *S.*, Right subclavian. *P.*, Pulmonary artery and its divisions. *S.C.*, Right superior cava. *L.C.*, Left superior cava. *V.*, Pulmonary veins. *T.*, Trachea. *L.*, Lungs.—*From a Photograph.*

The aorta (*A.*) comes off from the heart at the position usually occupied by the pulmonary artery—namely, it springs from the upper angle of the base of that ventricle which lies to the right of the interventricular groove. From its origin

it arches backwards over the root of the right lung, and runs down on the right side of the vertebral column, constituting a right aortic arch. Its concavity is connected by an imperious ductus arteriosus with the pulmonary artery, to the right of the bifurcation of that trunk. The right recurrent nerve passes backwards on the outer side of the ductus arteriosus, and, winding round the arch of the aorta, passes up to the larynx. From this it would seem that the ventricle which lies to the right of the interventricular groove is the representative of the normal left ventricle. Whether this conclusion would be correct or the reverse will be seen later.

The auricular portion of the heart is extremely irregular. On the right side there are two cavæ, inferior and superior, running normal courses; the latter receives the arch of the azygos major vein. These two cavæ open—the one below, the other above—into a cavity which occupies the position normally occupied by the atrium of the right auricle, but here, no more of the auricle is represented, there being on this side no trace of an auricular appendix.

Following the auricles round to the back, we see what appears to be the posterior surface of a normal left auricle receiving the pulmonary veins in the ordinary manner. We further see that this surface is crossed by a pervious left superior cava (L.C.), which is flattened out to a width of about half an inch, and is bound to the back of the auricle by epicardium. Following this auricle to the front, we find it continued into a typical left auricular appendix (L.A.A.), which is distinctly seen when the heart is viewed from the front. The pulmonary veins are quite normal.

Between the left appendix and the aorta, which it will be remembered occupies the position of the normal pulmonary artery, is a large mass (R.A.A.) resembling a right auricular appendix in appearance. This can be traced to the right

behind the aorta and pulmonary trunks to the atrium of the right auricle. As will be seen later, when the interior of the auricles is described, this is really the right auricular appendix which has grown across behind the arterial trunks, away from its normal position, and has reached the left appendix, by the side of which it now lies. So that the two arterial trunks are not embraced by the auricles, but lie to the right and in front of them. At this part also is seen a large triangular projection lying to the left of the first part of the aorta (F.); it looks like a third auricular appendix, but on cutting into it, it proves to be a mass of fat covered with epicardium.

The pulmonary artery (P.) lies at its origin behind and to the right of the aorta. It is narrower in calibre than usual, being under $\frac{3}{4}$ of an inch, while the aorta is over an inch (outside measurements). On the surface it springs from the base of the ventricular mass behind and to the right of the aorta. It runs upwards with a slight inclination to the left for about $1\frac{1}{2}$ inches, and then bifurcates into right and left branches; the left is the larger; it passes under the arch of the aorta to reach its lung. The ductus arteriosus is attached to the right branch.

The aorta, which curves to the right side of the vertebral column, gives off three branches; the first is a *left innominate* (I.A.), which divides into subclavian and common carotid the second is the right common carotid (C.); the third the right subclavian (S.).

There is a persistent left superior cava (L.C.), which is about one-third the diameter of the right superior cava; it receives the arch of a left azygos vein which comes over the root of the left lung, and then passes down in front of the pulmonary vessels, winds round to the back of the left auricle, and is finally continued into the coronary sinus, which opens in its usual position.

I opened the ventricles by anterior and posterior incisions made in each case along, and close to, the interventricular septum. The cavity of the left ventricle is somewhat smaller than that of the right; on transverse section it would be more or less circular; the right would be oval, not crescentic. The moulding of the right round the side of the left, usually seen, is not noticeable. The septum is not nearly so oblique as normal; it divides the ventricular cavity more directly than in the ordinary condition, and the bulging into the right ventricle is absent. The walls of the left ventricle are slightly stouter than those of the right, except near the base, where the thickness of the right exceeds that of the left, but this is owing to the addition of special thickening ridges at this part. In general, the difference in thickness on the two sides is not great. The wall of the right is much the more spongy in character; this is particularly the case as the cavity is approached. The general appearance of the walls of the right and left ventricles, when viewed internally, corresponds pretty closely to that of the walls of the same-named cavities of a normal heart.

The interventricular septum presents a striking anomaly—namely, it is deficient in its upper and anterior part; here there is a large aperture which admits a thumb. The aperture is placed close to the anterior wall of the heart, and is bounded below and behind by the thickened rounded margin of the septum. No artery springs from the left ventricle; and, besides the abnormal foramen in the septum, the only other opening in it is that of the left auricle, which is fairly normal, and guarded by a well-developed mitral valve, the muscoli papillares of which are not as powerful as usual. This orifice is placed closer to the interventricular septum than in the normal heart, owing to the absence of the origin of the aorta from the ventricle. It also might be noted that the large anterior flap of the mitral valve separates it, not

from the aortic orifice, but from the foramen in the septum, quite close to which it is placed.

On looking into the right ventricle, the opening of the right auricle, guarded by a tricuspid valve, is a prominent object, situated at the posterior part of the base; in front, and slightly to the left of this, the aortic orifice, with its semilunar valve, is seen; and crossing between the two openings, a prominent muscular ridge of large size. This ridge springs from the septum immediately behind the foramen, it courses to the right, lying between the aortic and auriculo-ventricular orifices, and runs down on the right side of the ventricle, where it ends midway between base and apex, by breaking up into columnæ carneæ. This ridge is highly developed, and it probably fulfils some important function—perhaps it helps to cut off from the rest of the right ventricle, during the contraction of the heart, that part with which the aorta is connected, and thus brings the aortic orifice into functional connection with the foramen in the septum, and through it with the left ventricle.

Even after a most careful search, no trace of the pulmonary orifice could be found within the ventricle until the artery was opened outside the heart and a probe passed down through it. The pulmonary orifice was then found nearly half-way between base and apex, and slightly posterior to the right margin of the heart. It is of small size, elongated oval in shape, and looks like a rather large depression between the columnæ carneæ, two of which, largely developed, bound it at the sides. From this obscure opening a tunnel runs up in the substance of the ventricular wall for an inch and a quarter towards its base; here, behind and a little to the right of the line of the aorta, the vessel becomes free, and appears as the pulmonary artery proper outside the heart. At this point, or immediately before it becomes free, it is guarded by a valve of an unusual shape. This valve was

evidently formed originally of three semilunar flaps; these have been prolonged upwards into the vessel, and have become united by their sides, reducing the lumen to a very small size, and by their union and prolongation forming a structure of a truncated funnel shape, with its narrow part running up for some distance into the trunk of the artery. The lumen of the valve is so much reduced that it only admits a wooden cylinder of five-sixteenths, or little more than quarter of an inch in diameter; while the aorta is fully three-quarters of an inch in diameter internally. Three sinuses of Valsalva are present above the valve.

The tricuspid valve guarding the right auriculo-ventricular orifice is well developed: one flap lies against the septum, chiefly behind but partly overlapping the foramen; a second flap is placed close to the right margin of the heart; while the third flap, which evidently corresponds to the infundibular flap of the normal valve, is of large size, and hangs down between the auricular opening and that of the aorta, which lies in front of it. The segments are quite normal, except that the infundibular one separates the auriculo-ventricular orifice from the aorta instead of from the pulmonary artery; this irregularity is explained by the fact that the aorta arises from the right ventricle, at the site usually occupied by the pulmonary artery—that is, it comes off from the upper and anterior part of the ventricle quite close to the septum. It is here guarded by a normal valve of three segments.

Below the aortic orifice there is a considerable portion of the upper and anterior part of the ventricle, which seems to be more or less distinctly marked off from the rest of the cavity, both structurally and functionally. It is bounded behind, first by the strongly developed muscular ridge which crosses the base of the ventricle between the aortic and auriculo-ventricular orifices, and more posteriorly by the large

anterior flap of the tricuspid valve. In front this space is marked off from the general cavity, to some extent, by a slightly developed muscular ridge, which, beginning at the septum, crosses on the anterior wall just in front of the aortic opening. On its left side is the foramen in the septum. This portion of the ventricle just described, even in appearance, seems to differ from the rest; it looks like a separate little chamber, incompletely partitioned off from the ventricle to form a vestibule for the aortic trunk. Into this vestibule on its left side opens the left ventricle through the foramen in the septum, from it above springs the aorta, while it itself forms the channel of communication between the two. The walls of this chamber are much thicker than those of the rest of the right ventricle.

As to the auricles:—On opening the right atrium it is seen at once that the septum auriculorum is almost completely absent, its only representative being a very thin ridge, about a quarter-inch high, which runs along the line of separation between the two auricles except above and in front; it is distinctly marked in the interval between the auriculo-ventricular openings. I do not think that this is merely a patent foramen ovale; it seems to be rather a deficiency of the primary inter-auricular septum. It will admit three fingers quite freely. Now it can also be seen that the cavity of the right atrium is continued to the left, behind the pulmonary artery and aorta, into the large appendix seen on the front of the heart between the aorta and the left appendix, and described above as the displaced right auricular appendix. At the lower part of the right auricle is seen the opening into the right ventricle, which appears quite normal; and between this and the aperture of the normal inferior cava, in the position usually occupied by the orifice of the coronary sinus, is found the termination of the left superior cava, opening under a well-developed single valve, the concavity

of which is directed downwards. The interior of the left auricle is normal but for the absence of the inter-auricular septum.

In such a case as this, three questions naturally suggest themselves :—First, How can the production of the abnormal conditions be explained? Secondly, How was the circulation carried on through the heart? And thirdly, What is the explanation of the physical signs observed during life? The method of production of most of the anomalies present can be readily explained by a reference to the development of the heart and great vessels; they may all be put down as errors in development or persistence of fœtal conditions. But as to their primary cause, I fear we can say but little.

The foramen in the interventricular septum is due to the persistence of a condition which is normal in the fœtus for a short time during the development of the heart. It will be remembered that this septum ventriculorum is built up from three sources: first, a strong ridge grows up from the bottom of the common ventricular cavity—the septum inferius; this is the chief factor. As it passes up in front of the orifice of the dividing auricle, it is joined by a prolongation forwards from the endo-cardial cushions which separate the common auricular orifice into two; this, with possibly another element added from the floor of the auricle, is the septum intermedium. When it has joined with the septum inferius, the ventricles are separated completely except above and in front; here there still exists an aperture corresponding in position to the abnormal foramen in the heart which I describe. This aperture is finally closed by the lower part of the septum which divides the aortic bulb into aorta and pulmonary artery; this, under the name of the septum superius, joins the septa inferius and intermedium, and completes the division between the two ventricles, forming the pars membranacea septi of the adult. The septum of the bulb under-

goes a peculiar rotation as it descends; above it is at right angles to the septum inferius; as it grows down it twists round, and finally runs in the same plane as this latter septum. Now, a foramen in the ventricular septum may be due either to deficient downward growth, or to imperfect rotation of the septum bulbi. I am inclined to think that the latter is the correct explanation in this case, for the septum seems to have undergone scarcely any rotation. The absence of this rotation also explains the origin of the aorta from the right ventricle. The septum bulbi in this case lies behind the aorta, and runs nearly transversely; had its right margin rotated forwards below it would have pushed the aorta over to the left, throwing it into connection with the left ventricle, and at the same time, by its union with the septum inferius, it would have completed the septum ventriculorum, and cut the aorta off completely from the right ventricle, throwing the part of that cavity which I described as the aortic vestibule into the left ventricle. From this it is apparent that the right ventricle in this heart is a compound cavity made up not only of right ventricle proper, but also of a portion of the left cavity, which should have been shut off from it by the descending septum bulbi.

The right aortic arch is, of course, explained by the persistence of the right instead of the left fourth arterial arch; in such a condition the first branch of the aorta will naturally be a left innominate, developed from the ventral root of the right arches, and the right carotid and subclavian will follow in this order.

The peculiar condition and position of the internal orifice of the pulmonary artery I am at a loss to account for unless it be due to a partial persistence of the foetal trabecular condition of the interior of the ventricle.

The free communication between the two auricles is explained as an almost complete absence of the auricular

septum. It might be unwise to suggest what part of the septum has failed, while a certain amount of doubt still exists as to its true formation; but it is perhaps worth remarking that there is complete absence of the partition above and in front, while it is best developed behind and below between the two auriculo-ventricular orifices, where His describes the growth upwards of a crescentic fold (connected with the septum intermedium), which Born has not found.

The position of the two auricular appendices lying together at the left side of the base of the heart may be explained by the failure of the developing appendices to embrace the aortic bulb—that is, the arterial trunks—owing to the abnormal position of these to the right of their usual site. No special reference need be made to the condition of the left superior cava.

These are the explanations of the method in which the anomalies were brought about. I wish I could give some adequate explanation of the primary cause which produced these results. This, I fear, I cannot do. To me it appears that the great error in this case must be attributed to the septum bulbi; if it had developed normally, many of the irregularities would have come right. Still, there is seen throughout the whole organ a sluggishness of development which must have had some general inherited cause.

Regarding the second question—how was the circulation carried on in the heart during life?—it should be remembered that the patient lived to the age of twenty years, and during a great portion of that time was employed in a fairly laborious occupation. To enable her to live and work up to this age, the circulation within the heart must not have been so chaotic as a look at the organ would suggest. That the blood in the two auricles mixed to some extent I have no doubt (the hemiplegia following curetting of the uterus favours this view), but that the mixture was not very great I am strongly

of opinion. I believe that the blood returned from the lungs was drawn through the left auricle by the suction of the left ventricle, and that the impure blood returned by the cavæ was similarly drawn through the right auricle without much mingling of the two floods. That all the blood which entered the left ventricle must have passed out of it through the foramen in the septum is evident, as there is no other opening into the ventricle except the valved mitral orifice. It is also probable that this blood sent through the septum into the right ventricle did not mix to any great extent with the impure blood of that cavity, but, on the contrary, that it passed direct from the foramen in the septum into the beginning of the aorta, through the special compartment of the right ventricle which I have previously described as the aortic vestibule. The blood in the two ventricles must have been under a similar pressure during the contraction of the heart, owing to the free communication between them, and hence the fact that the wall of the right is almost as thick as that of the left ventricle.

The quantity of blood returned to the right auricle by the cavæ must, of course, have been the same as that which left the heart by the aorta. If this blood passed down to the right ventricle without much mixture with the contents of the left auricle, as I suggest, the right ventricle would have had to eject at each systole as much blood as passed through the aorta at each contraction of the ventricles. How it could do this it is difficult to see, if the aorta be filled from the left ventricle through the abnormal foramen, and if it be remembered that the orifice of the pulmonary artery was little more than $\frac{1}{4}$ of an inch in diameter, while that of the aorta was about $\frac{3}{4}$ of an inch. Still, I believe that it did get rid of this large quantity of blood by sending a small part of it into the aorta, and the greater part through the pulmonary artery, small though it be; for, as pointed out above, the

pressure in both ventricles must have been equal; the resistance offered by the pulmonary circulation was very small as compared with that offered by the general circulation; as a result, I think I am correct in saying that the flow through the pulmonary orifice must have been much faster than that through the aorta, and that consequently a larger quantity of blood could be sent into the pulmonary artery than might be thought possible at first sight. Thus the greater part of the blood collected in the right ventricle was sent off to the lungs for aeration; and notwithstanding the deficiencies in the septa, and the small size of the pulmonary orifice, the circulation was kept in a condition of moderate efficiency (see Fig. 2, which shows this, in diagram).

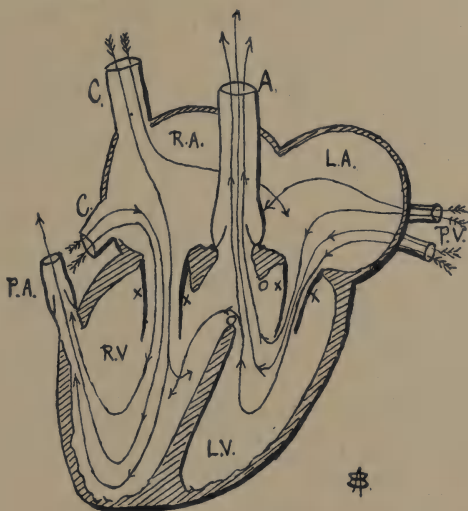


FIG. 2.—R.A., Right auricle. L.A., Left auricle. R.V., Right ventricle. L.V., Left ventricle. A., Aorta. P.V., Pulmonary veins. C.C., Venæ cavæ. P.A., Pulmonary artery. X.X., Tricuspid and mitral valves. Between O and O the foramen in the septum. The arrows indicate the probable course of the blood current. The crossing of arrows indicates a mixture of currents.

One word as to the cause of the physical signs. The loud,

harsh, systolic murmur was possibly due to the rush of the blood, driven by the powerful right ventricle through the narrowed pulmonary valve, and also by the rapid flow through the foramen in the septum. The loud, sharp, second sound, accompanied by a beat felt with the hand, may have been due to the superficial position of the aorta, and to the high pressure in the pulmonary artery acting on its valve.

DR. O'CARROLL said Dr. Birmingham had unravelled a difficult case in a much clearer manner than he had thought possible. It seemed to him that in faults of development we should look to some abnormal force as upsetting the normal course of growth. In this case a morbid sign, as distinct from a developmental fault, was seen in the adherent condition of the otherwise well-developed pulmonary valves—that is, in all probability the patient had an adhesive pulmonary valvulitis in utero. Such a condition, by the necessity for re-arrangement of the circulation which it entailed, seemed to him to account fairly for the halt in development of the ventricular and auricular septa. As to the cause of the rough systolic murmur heard during life, while he did not deny that it might possibly have been augmented by the double stream in the aorta, he felt pretty confident that the murmur was in greater part produced in the pulmonary artery, where the funnel-like condition of valves seemed eminently suitable for the production of a murmur; and he was confirmed in this view by the fact that the murmur was audible not merely in the aortic area of this case, but downwards towards the right nipple—that is, corresponding to the abnormal position of the pulmonary channel. Lastly, the accented second sound seemed more easily explained by the fact that the aorta had here, at its beginning, no right auricle and pulmonary artery to act as pads between it and the thoracic wall, but lay immediately behind it. It is interesting, from the clinical standpoint, to remember that there appeared, during the three years in which he kept her under observation, no reason why this patient should not have enjoyed a normal length of life had she not fallen a victim to a rather rapid attack of pulmonary phthisis.

DR. WOODS thought that some light might be thrown on the course of the blood-current by calculating if there was any difference (and if so, what?) between the pressures in the pulmonary arteries

and aorta respectively, by measuring the thickness and calibre of the vessels, and using the formulæ $P \propto \frac{T'}{r}$ or $\frac{t}{r}$ where P = blood-pressure, r the radius of curvature, and t the thickness of the wall.

PROFESSOR BIRMINGHAM, in reply, was glad to hear Dr. O'Carroll's opinion as to the production of the murmur and of the beat, which seemed very probable. His explanation of the cause of the anomalies in the septa of the heart was very ingenious, but he (Professor Birmingham) did not think that a valvulitis producing such a pulmonary valve as that in the specimen could be present before the time of normal completion of the septa. Regarding Dr. Woods' remarks, he thought it would be interesting to carry out his suggestion. Owing to the fact that both aorta and pulmonary artery come off from the same ventricle, the blood sent through both was driven by an equal pressure, but the resistance offered by the pulmonary system was much less than that offered by the column of blood in the aorta; consequently more blood could pass through the narrowed pulmonary valve than at first sight would appear possible.

THE LIGHT REFLEX ON THE RETINAL VESSELS.

By JOHN B. STORY, M.B., F.R.C.S.;

Surgeon to St. Mark's Ophthalmic Hospital.

[Read in the Section of Anatomy and Physiology, March 25, 1892.]

EVER since the invention of the ophthalmoscope, ophthalmologists have been familiar with the appearance of a light reflex or light streak on the retinal blood-vessels, but they are not, even at the present time, quite in accord as to the precise cause of this phenomenon. The history of the subject is briefly as follows:—

The very earliest observer, E. von Jaeger, attributed the reflex to a reflection from the walls of the vessels, though he subsequently, and I think erroneously, modified his theory by assuming that it came from the convex surface of the blood column, the vascular walls being, in his opinion, too transparent and too much of the same coefficient of refraction as the retinal tissue to produce the reflex. This theory of a reflection from the convex cylindrical surface of the blood, or of the vessel wall has been opposed by Loring^a in 1871, who holds that the light streak is caused by refraction of light transmitted from the illuminated background through the convex cylinder of the blood column, which in fact acts as a convex lens, and this view has again been advocated by Davis, in the "Archives of Ophthalmology," of which paper an abstract can be seen in the *Ophthalmic Review*, 1891, p. 88.

Loring supported his theory by the following arguments:—

1. The light streak is too broad to be a reflection from the convexity of the vessel or the blood column.

^a *Archiv. of Ophthalmology.* Vol. II., No. 1, p. 95.

2. The blood column is too transparent and non-reflecting to give so brilliant a streak.
3. If the blood column reflected the streak, the latter would be best marked when the vessels are full; but in venous pulsation it disappears when the vein is most full. (Loring describes venous pulsation as inducing a temporary stasis, and the crowding of the globules together renders the blood column so opaque that the light does not pass through it to cause the reflex.)
4. The state of the background affects the light streak—*e.g.*, it is absent in detached retina.

He supported his theory by the results of an experiment. A glass tube containing carmine solution placed in the bottom of a small box gives a light reflex when a mirror is placed beneath it, and none when the background is a non-reflecting surface—the illumination in both cases being that of ordinary ophthalmoscopic examination.

Loring's theory has been submitted to an elaborate refutation by Schneller.^a His view is that the reflex is an image of the ophthalmoscopic light reflected from the convex walls of the blood-vessels.^b This he establishes by a mathematical analysis of what the size of the reflection should be, the data being given—viz., the size of flame of lamp, the curve of mirror and diameter of blood-vessels, and the various distances.

The facts as observed by him agree with his calculations, as, for instance—

1. The reflex is narrower with short-focus mirrors and in myopic eyes.
2. The reflex moves with the rotation of the mirror.

^a Von Graefe's Archiv. XVIII., 1, p. 113

^b Jaeger's Archiv. No. 1.

3. The reflex is almost white—not red, as it should certainly be if Loring's view were correct.
4. The reflex is shortened in longitudinal diameter by backward curves of the vessels.
5. The absence in detachment is merely due to a want of perpendicularity of the vessel to the line of sight.
6. Besides, if Loring were right, the coefficient of refraction of the blood must be very much greater than that of the retina, which we have no reason to believe it to be; and the reflex could not be absent in vessels lying obliquely to the line of sight.

The reflex from the veins differs from that from the arteries, because the vascular walls are thinner, the vessel is more elliptical in shape, and lies deeper, and the walls of the veins are rougher and more wrinkled than those of the arteries.

Otto Becker^a treats of the same subject, but comes to no definite conclusion. He states that the vessels of a frog's mesentery have the power, which Loring assumes for the retinal vessels, of refracting transmitted light.

Jaeger^b defends his own view—

1. The reflex cannot come from a tissue beneath the vessel, for it remains unaltered by the background—*e.g.*, it is the same whether the vessel passes over the lamina cribrosa, the ordinary fundus of a dark or light-haired person, a white exudation, or a dark pigment spot.
2. The vessels cast an intense shadow on the underlying tissues, which can be observed easily under certain conditions.
3. The fact that the light streak is not red is enough to disprove Loring's theory.

^a Von Graefe's Archiv. XVIII., 1, 281.

^b Ergebnisse der Untersuchung mit der Augenspiegel. 1876. P. 51.

4. The convex vascular wall, again, cannot cause the reflex, for it disappears when the vessel is empty, as in embolism.

And if the wall were less transparent than the surrounding tissues it would be visible under normal conditions at the sides of the blood column—

- a. If the vessel wall caused the reflex by its coefficient of refraction being different from that of the tissues it would also be visible.
- b. And its borders would be marked by dark lines due to total reflection.
- c. And they would distort objects beneath.

Now, the light streak can be seen on a vessel right up to the border of the blood column of another vessel crossing above it at right angles. Jaeger allows that a small, colourless, hardly visible line can be seen at the edge of the blood column of the upper vessel, more distinctly when the latter is a vein.

Besides, if the vessel wall refracted the light, the streak could be seen through the wall of the upper vessel when, by movements of the head and mirror, it was invisible on the rest of the lower vessel.

Jaeger illustrates his views by experiments made with carmine solutions in two glass tubes, one crossing over the other at a right angle. When the tubes are immersed in a fluid of the same coefficient of refraction as the glass, the streak of light reflected from the lower tube is visible unaltered through the glass wall of the upper tube, and is only arrested by the column of carmine solution—in fact, behaves exactly as Jaeger asserts that the light streak on the retinal vessels does.

Schneller returns to the subject of the light reflex in v. Graefe's "Archiv.," XXVI., p. 71, and upholds his own view against Jaeger's. He asserts that both the arteries and

veins of the retina have walls which are visible with the ophthalmoscope at least in the larger vessels. Jaeger himself perceived a white line along the border of the vessels, which he erroneously thought to be a layer of white blood corpuscles. Schneller states that the thickness of the arterial walls compared with the radius of the lumen of the vessel is as 1 is to 3; that of the venous walls is as 1 is to 6 or 8. The inner surface of the vascular walls is smooth, the outer surface of the arterial wall is also smooth, while that of the vein is slightly rougher.

On each side of an artery which crosses over a vein a light yellowish-white line can be perceived of from one-tenth to one-sixth the breadth of the blood column. This cannot be the layer of white blood corpuscles imagined by Jaeger, for the thickness of a white blood corpuscle is too little to be ophthalmoscopically visible, and the white corpuscles do not form a continuous layer along the wall of either veins or arteries. This white line can be nothing else but the optical effect of the wall of the blood-vessel.

The vascular walls, therefore, are not absolutely transparent, but translucent, hiding underlying blood vessels like faintly muffed glass. The effect cannot be produced by total reflection, as no dark bordering line is present, such as can be seen in a dislocated lens, &c. The smallness of the vascular wall prevents any prismatic effect, and its translucency prevents it casting a visible shadow on underlying objects.

Schneller rejects Jaeger's argument drawn from the disappearance of the light streak when the vessel is empty, as it may be the absence of the cylindrical convexity of the wall which causes the obliteration of the reflex. Jaeger's second point—that the coefficient of refraction of the vessel wall is too nearly equal to that of the retinal tissue—has been dealt with above. The effect is caused by slight milkiness of the wall. Jaeger has subsequently advanced the hypothesis that

the refractive index of the blood is actually less than that of the retina; but this cannot be the case. The average index of the vitreous is, according to Krause, 1.3485, and that of the retina must be practically the same. Now Thomas Young (it is interesting to find that a German in 1880 has to go back to Thomas Young in 1801 for an authoritative statement on a point of physiology!) determined the index of blood serum to be 1.354—*i.e.*, actually higher than that of vitreous or retina.

Schneller has attempted to estimate the relative luminosity of the light streaks on the veins and arteries, optic disc, &c.

That on the arteries is brighter than that on the veins. On Jaeger's theory the reverse should be the case (by contrast with the colour of the blood the arterial streak should appear less bright than that on the veins), and the thicker arterial wall should make the arterial reflex less clear and sharp than that on the veins. Besides, the venous reflex should not be so irregular as it is, for the surface of the blood column is perfectly smooth.

All these difficulties are removed if we assume that the reflex comes not from the blood column, but from the vessel walls. The smooth arterial wall reflects a sharper image than the more wrinkled venous wall, and as it is also thicker, the reflex is brighter.

There are, then, only three theories suggested to explain the light streak on the retinal vessels. Which of these may be regarded as the correct one? There can be no reasonable doubt as to which is incorrect—*viz.*, Loring's. The arguments by which it is supported are unsound, and the experiments are inconclusive. All that they prove is that a cylindrical column of carmine solution has the power of refracting light—a statement which no one wishes to controvert; and Davis has merely shown that a blood column has the same power, which was observed by Von Becker years ago; all

these being observations made in air. As to the breadth of the light streak being too great to be a reflection from the convexity of the vessel, why the breadth of the streak depends entirely on the size of the source of light, which may be as large as the diameter of the pupil, or even larger. I can corroborate Schneller as to its varying directly with the magnitude of the source of light. Then, as to the transparency of the blood column, we all know it is not transparent, but only faintly translucent. Its shadows are visible, not alone objectively but subjectively in the well-known vascular figure of Purkinje.

As to the curious argument drawn from the absence of the light streak in the veins when distended by venous pulsation, I have not been able to observe the phenomenon, though I have carefully looked for it; and, if it were a fact, its explanation would be easier by the assumption of a change in the form of the surface of the vessel, making its surface no longer perpendicular to the line of sight.

The absence of the light streak in detachment of the retina is better explained by the slanting course of the vessels than by the change of background, while it is not improbable that the real agent may be something quite different from either theory, and the streak is not always absent. I have frequently observed it in typical cases of retinal detachment.

Loring's experiment is the most unsatisfactory proof of all. With a mirror placed beneath his glass tube, one sees the tube twice over—viz., the real tube and its image in the mirror. If these two objects are made to coincide the tube seen appears nearly double as bright as it would otherwise appear, but nothing at all resembling the light streak on the retinal vessels can be made out on it which cannot be seen on glass tubes over a dark background. A light streak can be made out easily enough on a glass tube with a less luminous background. In any case Loring's experiment should not

be made in air, but in some medium with an index of refraction pretty nearly equal to that of the glass tube and its contained fluid. If this be done (as in the tubes exhibited sunk in glycerine jelly) anyone can convince himself that the only reflex at all resembling that on the retinal vessels is that on the anterior surface of the tube.

Again, Schneller's observation that the light reflex moves with the rotation of the mirror is perfectly correct, and disproves Loring's theory completely, as does also the even more incontrovertible fact that the light streak is not red, but white. The absolutely unchanged character of the reflex, no matter what be the back ground—white, as in optic disc; black, as over pigment spots; or red, as over subjacent blood vessels—is also in my own experience an easily observed fact, and one vouched for by the acutest of all ophthalmoscopic observers—the late Ed. v. Jaeger, and it is completely destructive to Loring's theory.

When we come to a choice between Jaeger's second theory and his first—which is the one advocated by Schneller—the decision is not quite so easy; but I have myself no hesitation in electing to stand by the latter—viz., that the light streak is a reflex from the wall of the vessel.

Jaeger's experiment with the glass tubes is perfectly correct, so far as it goes, but does not disprove the theory that the light streak comes from the vascular wall, if we assume with Schneller that the wall does not differ appreciably in refraction from the retina, but is slightly opaque, and by its slight opacity causes the reflex. Or even if the reflex was caused by the refraction of the wall being high, still Jaeger's experimental argument could not be pertinent unless the whole thickness of the wall exhibited the high refraction. If it were only an extremely thin layer of the wall no visible effect would be produced on the light streak reflected from subjacent vessels.

But I can corroborate Schneller's observation that the walls of the larger retinal vessels are distinctly visible in ophthalmoscopic examination. In fact, since I have looked for them I have never failed to see them, and when in pathological conditions the vascular walls are thickened and opaque (in perivasculitis), the vascular light reflex is quite as marked a phenomenon as it is when the vessel walls are difficult to detect, as in young and healthy eyes.

A very striking and convincing observation can sometimes be made in young eyes which have that so-called shot-silk appearance in their retinae. The vascular light reflex can, in these eyes be seen as an intensification of the retinal reflex. As the light flashes over the retina it illuminates the arterial wall just in the same way as it does the shot-silk surface. The two reflexes are apparently caused by similar surfaces—that on the arteries being generally on a plane somewhat deeper than that reflected from the internal limiting membrane—if it be the internal limiting membrane which causes the shot-silk appearance.

I may add that in these eyes with that rare affection, detachment of the choriodea, I have seen the reflex.

REPORT OF COLLECTIVE INVESTIGATION,
IN THE ANATOMICAL DEPARTMENT OF THE
CATHOLIC UNIVERSITY MEDICAL SCHOOL.

*Report upon the Communication between the Ulnar and
Median Nerves in the Forearm. By P. J. Fagan, L.R.C.S.,
L.R.C.P.*

IN investigating the communication between these nerves I used the parts that were being worked in the dissecting room, and, as far as possible, adopted the following method:—Before the deeper dissection was commenced I cut the anterior head of the flexor carpi ulnaris and traced out the branches of the ulnar as far as the wrist. Subsequently I examined the median.

I collected 17 cases. In 4 I failed to find any communication, in 12 the nerves communicated beneath the ulnar artery, and in 1 a very large branch from the ulnar joined the median about three inches above the wrist. The usual arrangement was this:—As soon as the ulnar entered the forearm it gave off a branch which ran downwards and outwards towards the ulnar artery. From this branch one or more twigs arose, which entered the flexor profundus digitorum. The branch from the median emerged from the substance of the flexor sublimis digitorum, gave a twig to the flexor profundus digitorum, passed beneath the ulnar artery, and joined the branch from the ulnar, forming an arch with the convexity downwards. From this arch a twig arose, frequently by two heads, which entered the flexor profundus low down, and was distributed to the ring and middle finger portions of this muscle.

Report on the frequency of the Psoas Parvus and its Nerve Supply. By Martin J. Dempsey, M.B., &c.

I HAVE examined 26 subjects, with the following results:—

The muscle was present on both sides in 2; on one side in 5.

In 2 cases only was the nerve supply determined—in these it came from the first lumbar nerve.

Report on the Mode of Origin of the Branches of the Arch of the Aorta. By William Dargan.

I HAVE examined 14 aortic arches, and discovered but one marked irregularity in the origin of its branches—namely, in one case the left vertebral came off from the arch, opposite the interval between the left carotid and subclavian, and entered the transverse process of the fourth cervical vertebra.

NOTE ON OSSIFIED TRANSVERSE LIGAMENT OF ATLAS.

By A. BIRMINGHAM, M.D.;

Professor of Anatomy, Catholic University.

As might be expected, when the constant movement of the head is remembered, the ossification of the transverse ligament of the atlas is of rare occurrence. In the specimen which I wish to place on record the ligament was completely ossified, except at its extremities, which still remained fibrous, and were attached to the atlas in the usual way. The odontoid process, separated from the ossified ligament by a synovial sac, rotated freely within its ring, and the movements of the head seemed to be in no degree disturbed by the abnormal condition.

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